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Section of Obstetrics and Gynæcology

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[*March* 28, 1952]

Post-Partum Genital Tuberculosis

By ARTHUR M. SUTHERLAND, M.D., F.R.F.P.S.G., M.R.C.O.G.

IN recent years there has been a general revival of interest in the subject of genital tuberculosis in women. While much attention has been paid to the association of this condition with infertility, very little has been written about the occurrence of genital tuberculosis in the post-partum period.

The earlier literature contains a number of references to the occurrence of genital tuberculosis after childbirth or miscarriage, and a notable feature of these reports is that the cases described were almost invariably acute in onset, widespread and fatal. The subject is discussed and the literature is fully reviewed by Fruhinsholz and Feuillade (1924), Daniel (1925), Jameson (1935), Georgescu, Petrescu-Cotovu and Georgescu (1949), and Jedberg (1950), and in two previous communications by the writer (Sutherland, 1943 and 1950).

PRESENT CASES

I am at present in the early stages of an extensive investigation into various aspects of genital tuberculosis in women. Thanks to the co-operation of my colleagues in the West of Scotland, I have been able to see a large number of cases in a relatively short period of time. Since commencing this work about eighteen months ago, I have, to date, fully investigated 60 cases in which there has been histological and usually bacteriological proof of the condition, and there are still at least 40 further cases awaiting admission.

Among these 60 cases there are no less than 6 in which it is likely that the tuberculous lesion developed during pregnancy or in the post-partum period, although it is not possible to say with certainty that this was so. I shall now comment briefly on each of these cases in turn.

Case I.—This case is by far the least convincing of the group. Four years before seeing me she was delivered of her only child, who is still alive and healthy. Within a few weeks of the delivery she complained of lower abdominal pain, which has since persisted. Two years later she had a laparotomy in another hospital, when a tuberculous right tube and ovary were removed. When I saw her, she had well-marked left-sided adnexal swelling, and curettage revealed tuberculosis of the endometrium.

Case II.—The second patient has had 2 abortions, both occurring in the fourth month of pregnancy. She was referred to hospital with a history of almost constant vaginal bleeding since the second abortion, eight months previously. On pelvic examination she was found to have slight, right-sided adnexal thickening, and curettage showed endometrial tuberculosis to be present.

Case III.—The third patient was admitted to my unit in labour at the 30th week of her first pregnancy, having been well in all respects during the antenatal period. She delivered herself spontaneously of a 2 lb. 15 oz. child, who died three days later. Unfortunately, no post-mortem examination was performed. In the early days of the puerperium, the patient had a slight irregular pyrexia for which no cause could be found and which settled in about a week.

She was readmitted four months later with a history of left-sided lower abdominal pain and irregular vaginal bleeding, starting shortly after dismissal. On pelvic examination the only abnormality found was extensive infection of the cervix. Dilatation and curettage and cervical biopsy were carried out and both endometrium and cervix showed tuberculous infection.

Case IV.—The fourth patient has an interesting history. I first saw her in the later months of her ninth pregnancy, her 8 previous pregnancies having terminated either in stillbirth or in neonatal death. I was unable to find any cause for this unfortunate series of events, so I performed lower uterine segment Cæsarean section shortly before term. At operation there was no evidence of tuberculous infection in the pelvis.

The patient was readmitted three months after delivery with a history of continual vaginal bleeding since dismissal. Pelvic examination revealed no abnormality apart from a cervical erosion. Dilatation and curettage and cervical biopsy were carried out and only the endometrium showed evidence of tuberculous infection.

The child was alive at birth, although feeble, and weighed 5 lb. Progress was at first satisfactory and B.C.G. inoculation was advised, but the mother would not agree to this. When seen recently, at the age of 18 months, the child was found to have developed a primary tuberculosis of both lungs and a swelling of one knee which is probably tuberculous in nature.

Case V.—The fifth patient has had one pregnancy which terminated in a three months' abortion. Following this she had profuse, irregular, almost constant vaginal bleeding. Two months after the abortion she was admitted to hospital and no gross pelvic lesion was found. Dilatation and curettage revealed the presence of endometrial tuberculosis.

Case VI.—The last patient had a two months' incomplete abortion, accompanied by moderate vaginal bleeding. Dilatation and curettage were performed and the curettings were found to consist of products of conception and fragments of endometrium showing tuberculous infection. Three months later, well-marked bilateral adnexal thickening was found to be present, although this was not apparent at the time of the abortion.

In all of these 6 cases the condition was relatively mild throughout. No patient was acutely ill at any time and the clinical picture bore no resemblance whatever to the acute post-partum cases described by other writers. X-ray examination of the chest and abdomen was carried out in each case. One patient was found to have a quiescent pulmonary lesion and all other films were negative. Investigation of a catheter specimen of urine, including guinea-pig inoculation, was negative in each instance.

Guinea-pig inoculation with part of the tissue obtained by curettage was carried out in each case. A positive result was obtained from 5 of the 6 specimens. In all 5 the tubercle bacillus was found to be of the human type. The literature contains few references to the type of tubercle bacillus associated with genital tuberculosis in women. In the important monograph by Jedberg (1950) 191 cases were investigated and 179 were bacteriologically positive. 166 were of the human type, 12 were of the bovine variety and the last was unidentified owing to secondary infection.

Five of these patients have been treated with a course of streptomycin and PAS and the sixth is about to start treatment. The dosage employed has been 1 gramme of streptomycin daily and 3 grammes of PAS four times daily for 84 consecutive days. At present it is too early to assess the results of treatment in these patients. This aspect forms part of a much larger investigation and a short paper on this subject will be presented at the Thirteenth Congress of Obstetrics and Gynæcology at Leeds on July 11, 1952. Of necessity, this will be a preliminary report and the long-term value of these drugs will only be apparent at a much later date.

DISCUSSION

The only recent reference that I was able to find on cases of this type is the paper by Russell, Jackson and Midgley (1951). In this communication, the results of a personal study of 40 cases of pelvic tuberculosis in women are analysed. The condition manifested itself shortly after abortion or delivery in 4 of their cases, an identical incidence to that of my own series to date.

In one of their cases there was a history of tuberculous peritonitis which developed during her last pregnancy, and a second patient was admitted with tuberculous peritonitis twelve days after a three months' abortion. The third and fourth patients developed pelvic tuberculosis a few weeks after an abortion; in each case death resulted from tuberculous meningitis. It will thus be apparent that their cases were of a more acute type than those in my own series.

In a recent communication (Sutherland and Garrey, 1951) a survey was carried out of all histologically proved cases of genital tuberculosis occurring in the Royal Samaritan Hospital for Women, Glasgow, during the twenty-year period from 1930 to 1949. During this time, 65,943 patients were admitted to the Hospital and genital tuberculosis was discovered in 369 of these.

This incidence of 0·56% of all admissions to hospital is in reasonable agreement with the figures recorded in the recent literature from many centres throughout the world. It does not in any way support the view, which has frequently been expressed to me, that the incidence of female genital tuberculosis in Glasgow is unusually high.

In this series there were 315 married patients, of whom only 47 were parous. A careful analysis of the case records of these 47 patients revealed only 5 in which the symptoms appeared to date from childbirth or miscarriage. In no case was the presence of genital tuberculosis demonstrated earlier than seven months after delivery and the average interval for the group was well over a year. In addition, endometrial involvement was only demonstrated in 2 cases. In view of these figures, the incidence of 10% of probable post-partum cases in the present series is surprisingly high.

It is, of course, possible that the genital tuberculosis developed during the pregnancy in one or more of the present cases. In the last case, it is not unlikely that this did occur, as suggested by the presence of products of conception and endometrial tuberculosis in the same slide. There was, however, no history of any upset of any kind during the pregnancy preceding the discovery of the tuberculous lesion in any of my own patients. Herring and King (1950) recently described a case of tuberculosis of the endometrium and cervix which probably developed during pregnancy, and a number of other possible examples are described in the literature.

Even more unlikely is the possibility that the uterine tuberculosis preceded the pregnancy, but it would not be possible to prove or disprove this point on the evidence available. Although the occurrence of pregnancy in a tuberculous tube is by no means an extreme rarity, proved cases of pregnancy developing in a known tuberculous uterus are seldom reported. This is borne out by the follow-up results from infertility clinics where cases of endometrial tuberculosis are comparatively frequent. Quite a few cases of pregnancy developing in a tuberculous uterus have been described by the earlier writers, but the facts available about most of these cases are very incomplete, and, in the majority, proof that the uterine tuberculosis preceded the pregnancy is lacking.

More recently several authors have described cases of this type, although the diagnosis of tuberculosis has usually been made on histological grounds alone. Sharman (1952) described a case of endometrial tuberculosis where the histology was typical but no bacteriology was carried out. Subsequent endometrial biopsies were negative and eleven years later she became pregnant and was delivered of a healthy child. Post-partum endometrial biopsies showed no evidence of tuberculosis. No treatment was given in this case.

Kullander (1952) described a case of endometrial tuberculosis which was discovered after an abortion. Following treatment with PAS alone, the patient became pregnant and was later delivered of a healthy child. Stallworthy (1952) recently encountered a case in which pregnancy developed after tuberculous endometritis had been proved by positive culture. Other examples of this condition have been recorded recently by Eriksen (1947) and by Murray (1950).

In view of the infrequency of these reports and the fact that bacteriological proof was sometimes lacking, it is apparent that, while pregnancy can develop in a tuberculous uterus, this event is still something of a rarity. It is as yet too early to say whether the widespread use of streptomycin and PAS will make any appreciable impression on the very substantial amount of infertility associated with endometrial tuberculosis. It is my intention to follow up my own patients as long as they can be persuaded to attend and I await the long-term results with great interest.

Finally, I should like to emphasize that my figures suggest that the development, or at least the discovery, of a chronic form of endometrial tuberculosis in the post-partum period is by no means uncommon. In all cases in which there is persistent bleeding or abdominal pain after childbirth or miscarriage, the possibility of pelvic tuberculosis should be considered and suitable diagnostic measures should be undertaken.

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Post-Partum Genital Tuberculosis.—LOIS E. HURTER, M.D.Lond., M.R.C.O.G.

Case report.—A primigravida aged 18 was first seen at the 10th week of pregnancy with erythema nodosum. No abnormality was found on examination and investigation for a focus of tuberculosis was negative.

The antenatal period was normal apart from a loss of weight of 3 lb., after an initial gain. After a spontaneous premature delivery of a 4½ lb. child, the puerperium was, for ten days, uneventful. On the tenth and subsequent days she ran a temperature of 99–100·4° F. She had no complaint and investigations were again negative apart from a *B. coli* urinary infection. At this time no mass was felt in the abdomen other than the involuting uterus. After a course of Sulphatriad the urine became sterile but the pyrexia persisted. Seventeen days after delivery a mass was first noted to the right of the involuting uterus. During the next few days this mass increased in size until it almost reached the umbilicus, it was more circumscribed, slightly tender and pushed the uterus to the left. The patient still had no complaints.

At laparotomy twenty-four days after delivery, the peritoneum was studded with miliary tubercles, the mass consisted of a distended fallopian tube with ovary and intestine adherent to it. The surface of all the pelvic organs was covered with tubercles, there was very little free fluid. The diagnosis was confirmed histologically and bacteriologically.

Streptomycin and PAS were started, the pyrexia settled within forty-eight hours, improvement has been maintained. The patient now three and a half months after delivery has gained weight, the mass has decreased in size and she is in a special unit for the treatment of abdominal and pelvic tuberculosis. The infant has made good progress and has been vaccinated with B.C.G. vaccine.

Toxæmic Accidental Hæmorrhage
Observations On Its Ætiology, Phases and Treatment

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THIS contribution concerns what is termed toxæmic accidental hæmorrhage and its treatment, in a series of 100 cases, 7 of whom died and the remainder were followed up for their subsequent history.

The definition used for more than thirty years in the Rotunda Hospital has been "antepartum hæmorrhage (i.e. after the 28th week of pregnancy), excluding placenta prævia, associated with albuminuria and/or other evidence of toxæmia", and I have written to a significant number of representative centres who state that this definition is universally acceptable. This definition suffices for hospital clinical reports, but the same clinical phenomena occur earlier than the 28th week.

From my experience in the Rotunda certain facts emerge:

(1) That toxæmic accidental hæmorrhages occur four times oftener in multiparæ and in rapidly multiparous women than in primigravidæ, or in women with more widely spaced pregnancies.

(2) That we too readily label accidental hæmorrhages as of toxæmic origin because there is (a) albuminuria, or (b) œdema, or (c) both. About 25% of the cases we call "toxæmic" are, in reality, examples of the shock albuminuria syndrome I refer to as so-called toxæmic accidental hæmorrhage.

(3) That antenatal care does little to prevent the complication, although it does reduce the incidence and severity of other toxæmic conditions.

(4) That to continue the present highly conservative and largely (or at least late) non-transfusion treatment offers no possibility of improvement in the fœtal and maternal death-rates.

(5) That shock is the commonest cause of maternal death—whether immediate or later.

(6) That two phases exist in many accidental hæmorrhages. In the first the baby is alive and the shock slight; in the second phase the baby is dead, the shock definite, and the urine contains albumin.

(7) That the incidence of post-partum hæmorrhage is no greater than average in these cases.

In order to make my definitions clear, so-called toxæmic accidental hæmorrhage is found in association with albuminuria although the patient did not have toxæmia before the hæmorrhage. In other words, the albuminuria is a shock albuminuria secondary to the hæmorrhage. Genuine toxæmic accidental hæmorrhage is an accidental hæmorrhage occurring in a woman who is toxæmic.

Incidence.—As calculated against almost 91,000 deliveries since 1919, and in spite of increased antenatal care our incidence has remained constant—about 0·6% (6 per 1,000).

Parity, age, period of pregnancy and fœtal loss.—The parity varied from 1 to 22, averaging 6·4; the commonest parity was 8; the average age was 34 years, and only 20 were primigravidæ. The complication usually occurred at the 34th week.

The stillbirth rate (excluding neonatal deaths) was 75%, as calculated against all grades of severity. In the severely shocked accidental hæmorrhages the fœtal loss was almost 100%; in the truly toxæmic group the fœtal loss was 82%; in the so-called toxæmic group, only 21%.

The degree of placental separation.—Generally there are no reliable clinical criteria to indicate how much placenta has separated or is damaged unless this is sufficient to produce the well-known grave constitutional upset in the mother, or to kill the fœtus *in utero*. In many women, however, there are warning signs and symptoms, and this transition period I have termed Phase I. Gross abruptio (or ablatio) placentæ, whether occurring initially or after some early and small warning separations (Phase I), rapidly produces what I have termed Phase II, which is characterized by shock, an almost 100% incidence of sudden intra-uterine fœtal death, and a maternal mortality risk of at least 10%.

Phase I is frequently associated with crampy abdominal pains, occasional slight vaginal bleedings, and irregularities of the fœtal heart. The pains and cramps in this Phase are often mistaken for true labour. Transient albuminuria, and abnormal fluctuations in (or a downward trend of) blood pressure are common; slight temporary shock may occur with small, localized placental separations and its associated albuminuria clears within the next few days.

These Phase I symptoms and signs are not given sufficient emphasis at present and are often confused with the possible onset of labour. It is in Phase I that Cæsarean delivery can save fœtal life and avoid the development, or the increase, of shock. Unfortunately Phase I is likely to be overlooked unless the woman is being closely watched in hospital.

Types of accidental hæmorrhage.—The concealed, revealed, and combined types are described, but it is wrong to consider blood loss as the only or chief cause of death. The most dangerous groups were the concealed (3 with 2 deaths) and combined (88 with 5 deaths). The average delay between the initial concealed hæmorrhage and admission to hospital was six hours, and the average blood loss was 2 pints (1½ internal).

CLINICAL FACTORS OF IMPORTANCE

The onset of labour.—The only accurate method of deciding whether or not the woman is in labour is by vaginal examination. Formerly we have frequently mistaken the false pains of Couvelaire formation, or "cracking" or fissure formation in the peritoneal coat of the uterus by increased intra-uterine tension, for true labour. Only 24 of our 93 survivors (26%) were in labour on admission, and 15 (62%) of their babies were stillborn; 69 (74%) were *not* in labour—all the maternal deaths were in this group—and 62 (90%) of these babies were stillborn. This is highly suggestive that the absence of, or the inability to induce, labour is indicative of the seriousness of the case.

Lapse of time between placental separation and the application of active treatment.—In our 7 fatal cases there was an average of six hours' delay between the hæmorrhage and application of active restorative treatment, and a further average delay of three and a half hours (i.e. a total of nine and a half hours) before delivery was accomplished. It is most important that this delay between the hæmorrhage and delivery should be reduced.

The assessment of shock.—There does not appear to be any constant or direct relationship between the amount of blood loss and the rate of increase of the shock, but shock deepens until the uterus is emptied and delay is harmful. When anæmia complicates genuinely toxæmic accidental hæmorrhage the advent of shock is more serious.

Blood pressure.—This is an invaluable guide in the detection of Phase I and in the conduct of the case in Phase II. A persistently falling blood pressure in Phase II is *very* serious and usually associated with a progressively rising pulse rate and a falling pulse pressure, with approximation of systolic and diastolic readings. These signs indicate myocardial involvement with flagging circulation, deepening shock, increasing tissue anoxia, and the establishment (in varying extent) of possibly irreversible changes in the kidneys and elsewhere.

To establish a true picture of the case detailed observation is required half-hourly during the later stages of Phase I and in the acute stage of Phase II. In shocked chronic hypertensives a fallen blood pressure may be above 130/80 mm.Hg, and this is misleading.

Œdema.—We believe an excessive weight gain in pregnancy is indicative of early toxæmia from occult œdema. Œdema is uncommon in the genuine chronic hypertensive, and in the but slowly increasing toxæmia; it is usual in the more severely toxæmic women who may, or may not, suffer from chronic hypertension. We believe œdema is harmful.

In our 100 cases of toxæmic accidental hæmorrhage there was no œdema in 53, it was slight in 26, moderate in 15, and massive in only 3 (the condition of the remaining 3 cases was unfortunately not noted).

Albuminuria.—The presence of genuine albuminuria (pus, &c., excluded) in the urine either before or during the crisis is a definitive *sine qua non* of either the genuine or the so-called toxæmic accidental hæmorrhage and, because albuminuria is so easily detected and so commonly present in both pregnancy toxæmia and accidental hæmorrhage, most accidental hæmorrhages hitherto have been considered of "toxæmic" origin. 72 cases had positive evidence of pre-existing toxæmia, but the remainder showed neither albuminuria nor other signs of toxæmia. Although albuminuria may be absent *before* the accidental hæmorrhage it quickly appears following the onset of shock, presumably due to renal anoxia. Shock albuminuria usually disappears twenty-four to thirty-six hours after delivery.

Albuminuria and urinary output are correlated, and both are connected with the shock, the blood pressure disturbance, and the renal damage. The lapse of time from the first bleeding to admission to hospital bears no constant relationship to either and although shock increases with delay in emptying the uterus, extensive renal shock-damage may have occurred within the one to one and a half hours following the abruptio.

TREATMENT

We pay too little attention to the frequent (half-hourly) blood pressure trend and hourly urinary output during the vitally important first hours of Phases I and II in accidental hæmorrhage. Acute renal failure (renal cortical necrosis or lower nephron nephrosis), or the infrequent pituitary necrosis, develop from shock, and may claim victims some days after the initial accident. The urinary output and albuminuria should be estimated, together with the blood pressure, &c., at least hourly during Phase II, and further treatment must be based upon the response to conservative and restorative measures.

In 1914 Tweedy and others abandoned *accouchement forcé*, and he suggested plugging the vagina. FitzGibbon added his touches, advocating puncture of the membranes, and his treatment towards emptying the uterus has been largely followed since 1919. Conservative treatment has, in fact, held sway, with the occasional Cæsarean section and sporadic employment of blood transfusion. For several generations we have given saline intravenously and morphia hypodermically (nowadays it should be given intravenously), followed by puncture of the membranes and injections of Pituitrin or Pitocin to induce labour and encourage the uterus to empty itself, shut its muscular network and prevent both further fresh bleeding and the reabsorption of altered (and possibly toxic) blood fluids. Today this treatment offers no better results than those obtained in 1919. I do not wish to convey the impression that my predecessors, and the present and past Masters of the other Dublin

Maternity Hospitals, have not used both blood transfusion and Cæsarean section, but we all have been afraid to utilize this treatment to its full extent and have thus allowed shock to develop or increase. We must now either continue to accept an overall maternal mortality rate of nearly 8%, and a 75% stillbirth rate, with conservative treatment, or change our approach to the problem.

Shock, especially when genuine toxæmia is present, is a much more potent factor than is realized and only 1 in 4 of our patients were in labour on admission—all the fatalities occurred in the non-labour group.

Shock and absence of labour are closely related, as are the output of urine, the blood pressure and renal damage. In view of the importance of shock in preventing the onset of labour, and of its power to promote or increase kidney damage, we now question the wisdom of attempting to induce labour by puncture of the membranes if even only slight shock is already present, and we regard lower segment section as less likely to promote shock than the strain of labour.

Until a year ago we attempted to induce labour by puncture of the membranes in *all* degrees of shock, and are now satisfied this was often foolhardy. Maternal deaths occurred when shock was severe or moderate, whether or not the membranes were punctured, and the fœtal loss was also closely related to the degree of shock. The non-puncture of membranes group were the milder cases where, mostly, no active treatment was needed and labour was present.

We must now choose between our time-honoured treatment, with its overall 8% maternal and 75% stillbirth rate, or try to improve our results by earlier blood transfusion and abdominal section. In general, we have hitherto employed Cæsarean delivery, with or without hysterectomy, only in the most severely shocked patients who have not responded to preliminary restorative treatment and puncture of the membranes. Initially the majority of my own laparotomies for accidental hæmorrhage were desperate measures, performed belatedly, and without blood transfusion, as now used; yet only 2 women died. Recently the results from my earlier applied laparotomies, following adequate blood transfuion, have been encouraging both from the maternal and fœtal survival figures.

Since the original 100 cases were collected and analysed more have occurred, and we have now performed 18 laparotomies in 121 cases of toxæmic accidental hæmorrhage, with 2 deaths (11·1%). 15 of these 18 women were delivered by Cæsarean section only, with one maternal death (6·6%), a case early in the series, and Couvelaire lesions were present in 8 (53%). A further 3 women were treated by Cæsarean hysterectomy, with the loss of one mother, and Couvelaire lesions were present in these 3 women. Thus, Couvelaire lesions were present, with severe shock and oliguria in 11 of the 18 cases treated by laparotomy (61%). The stillbirths were 100% when there were Couvelaire lesions, but 4 live babies were delivered in the 18 abdominal deliveries (live birth-rate 22·2%) as against the usual 18% rate in similar cases treated conservatively. The 4 live births were in 3 Phase I and 1 early Phase II cases.

The diagnosis of Couvelaire lesions.—I am unable to diagnose accurately the existence of a Couvelaire lesion, or of cracks or fissures in the serous coat of the uterus, with bleeding within the peritoneal cavity, but it appears that these lesions are commoner than we suspect (in 61% of the cases delivered abdominally) and are probably present when:

(1) The shock is profound or increasing (blood pressure, pulse, &c.).
(2) The urinary output is markedly lessened and its albumin content increasing.
(3) The delay between the hæmorrhage and the application of active treatment is more than two to three hours.
(4) The woman is not in labour.
(5) There is little or no improvement following a 3-pint blood transfusion.
(6) Toxæmia is present, as distinct from shock albuminuria.
(7) When any especially tender area exists in the uterine wall.

CÆSAREAN SECTION *versus* CÆSAREAN HYSTERECTOMY

We cannot, from such particulars as we possess, or those collected from elsewhere, give a definite opinion on how many women who were delivered vaginally and recovered, or died after vaginal delivery without post-mortem being performed, had Couvelaire lesions, uterine wall fissures, and/or intraperitoneal hæmorrhages, but it is likely that a considerable number of women with Couvelaire lesions delivered themselves vaginally and recovered. This should make us hesitate at laparotomy before removing a uterus containing Couvelaire lesions. Our present routine is to empty the uterus, sew up the lower segment incision, and do nothing more. If cracks are present in the serous coat of the uterus we oversew them with fine catgut to control oozing. Undoubtedly Couvelaire lesions are productive of shock so long as the uterus is distended by its contents, but the shock increase ceases following emptying of the uterus, and there is no danger of post-partum hæmorrhage if the uterus is conserved. There has been no maternal death in the last 16 cases treated by abdominal section, although Couvelaire lesions were present in 56% of them. In the 103 cases treated conservatively, without laparotomy (many being the milder cases) there were 5 deaths (4·9%).

My inclination *initially*, if even a slight Couvelaire lesion was present, was to perform a Cæsarean hysterectomy. Even now my figures are insignificant, and I have received little help from the

additional figures taken from the carefully compiled figures from the Reports of my predecessors, and from those of the present and past Masters of the two Dublin sister Maternity Hospitals and elsewhere. I now believe that hysterectomy following Cæsarean section not only adds to the risk of operation but may tip the scales adversely when the woman is already shocked. Emptying the uterus by Cæsarean section is a possibility under local anæsthesia, but hysterectomy necessitates a spinal or general anæsthetic.

It appears that lower segment Cæsarean delivery is:

(1) Denied unnecessarily, or used too late, in Phase I.
(2) At least as helpful by itself as in conjunction with hysterectomy.
(3) Indicated earlier in Phase II, when shock and oliguria are present.
(4) Should be *preceded* by early and adequate blood transfusion (3 pints).
(5) Is sufficient, without hysterectomy, unless uterine rupture is present (as in one of my cases).

MATERNAL DEATHS

I next turn to the 7 deaths (5·8%) in my 121 cases of toxæmic accidental hæmorrhage. All were of the combined or concealed type; none of these women were in labour on admission; all reached Phase II shock, with a dead fœtus *in utero*; only 1 death occurred in a primigravida; 4 had clinically demonstrable œdema; none were treated early and adequately by blood transfusion and, when Cæsarean section was employed, it was as a desperate and belated measure. Couvelaire lesions were present in 5, absent in 1, and in the 7th (a spontaneous delivery) we have no data. 4 were acute toxæmics with shock added; 2 were severely toxæmic and shocked in addition to being chronic hypertensives, and one was a true (non-toxæmic) accidental hæmorrhage with marked shock secondary to the blood extravasation (a Couvelaire lesion was present in this case).

There are great possibilities in the work being carried out elsewhere on the thromboplastin phase in toxæmic accidental hæmorrhage and tissue damage, and it may be that this non-clotting blood condition may throw light upon the hæmorrhages occurring in the liver, the kidney and between the uterine muscle bundles. It is also a possibility that some of the post-partum bleedings described by others may be influenced by this factor, but we must record a total absence of post-partum hæmorrhage in our series, even when Couvelaire lesions have been present. It is likely that the slight but persistent, dark, non-clotting terminal bleedings associated with trauma (difficult forceps, tears of the uterine wall, incomplete third stages, &c.) are also connected with tissue damage, shock, and the thromboplastin phase.

Does the hæmorrhage in these cases begin in the placenta or in the decidua? Close naked-eye examination of these placentæ immediately following their delivery at Cæsarean section suggests the placental origin. One of my sections shows miniature intra-placental eruption areas, pear-shaped, with the narrow orifice pouting into what was the choriodecidual space. It is not suggested that the bleeding (2 pints or more) is from the fœtal circulation—nor that all accidental hæmorrhages have a similar mode of production—but it seems that these intra-placental bleedings or eruptions, especially if they are near the maternal surface and connected with a sufficiently large placental vessel could be the starting point of choriodecidual separation and thus allow the escaping maternal blood to become an hydraulic force with unknown consequences.

Colour transparencies of placentæ from cases of accidental hæmorrhage, and a few slides of figures concerned with the cases were then shown.

CONCLUSIONS

So long as we persist with our time-honoured concept of treatment, and neglect to apply oxygen by inhalation and through blood transfusion to counter shock, and delay the removal of the cause of the shock by earlier Cæsarean section—our maternal mortality rate will remain static. Everyone in charge of a large maternity service is confronted with this problem. We must begin at the beginning—strengthened by our knowledge of the past and present. We ourselves, our nurses, students and postgraduates, must learn to recognize that, in about 10% of toxæmic and so-called toxæmic cases, Phase I exists for some hours during which period the child is probably alive, and that Cæsarean section will not only save its life but prevent the full development of Phase II. In Phase II the child is dead, and some unknown degree of irreparable shock damage to the mother has either occurred or is in progress. This necessarily implies that we must be alive to these possibilities, and have at our immediate command a full and adequate blood transfusion service with more rapid transport of the patient into hospital. We must, in fact, discard conservatism in favour of earlier blood transfusion and resort to Cæsarean section. Puncture of the membranes, with its delay before the onset of labour—and the delay in labour itself—necessarily involves loss of precious time before the uterus is emptied, and today earlier blood transfusion and earlier Cæsarean delivery are indicated.

Time alone can prove whether these views are correct or harmful. The fact that in the past the denial of abdominal delivery in toxæmic accidental hæmorrhage was correct or otherwise does not matter. Our present knowledge of blood transfusion, our command of anæsthesia, and even our Cæsarean section technique, are so much better than some years ago that the change in treatment is wholly justified.

Some Features of Placental and Decidual Vessels in Relationship to Toxæmic Accidental Hæmorrhage

By NINIAN MCI. FALKINER, Sc.D., M.D., D.P.H., F.R.C.P.I., F.R.C.O.G.

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PREMATURE separation of the normally implanted placenta when it is associated with shock and renal involvement describes the condition known as toxæmic accidental hæmorrhage.

Couvelaire's description in 1912 is an accurate account of the clinical course of the condition.

Normal implantation of the placenta carries with it two implications: (*a*) Normal site, i.e. the anterior or posterior wall of the upper segment of the uterine cavity; (*b*) Normal relationship between the chorionic elements of the placenta and the decidua.

The very factors which facilitate the normal separation of the placenta in the third stage of labour make premature separation possible.

The circulation of maternal blood through the placenta is maintained by arterial pressure, the blood passing by means of the utero-placental coil arteries into the intervillous space.

These tortuous arteries traverse the anatomical layer of the decidua spongiosa, having left the musculature of the uterus.

That arterial pressure can effect a placental circulation is proved by the ectopic pregnancies that reach full term (but note that in such cases there is no decidua and no coil arteries).

In normal pregnancy the painless contractions of the uterus have in all probability a profound effect on the flow of blood through the placenta.

The exact mechanism of this effect is difficult to work out, but probably these contractions, which increase the pressure in the intervillous space, do not impede the venous channels all at the same time; the coil arteries' supply to the placenta would be diminished, and thus a thorough emptying out of the maternal blood into the uterine veins would result, the period of relaxation allowing arterial blood to flow freely again.

In accidental hæmorrhage does placental separation cause the hæmorrhage; or does the hæmorrhage cause the placental separation?

In post-partum hæmorrhage and placenta prævia the separation causes the hæmorrhage.

I do not think that it is likely that placental separation precedes accidental hæmorrhage. We know that partial separation can occur associated with localized ante-partum hæmorrhage which is most likely causal.

My conception of the steps that take place in sudden and complete ante-partum separation of the placenta is as follows: The uterine muscle goes into spasm, affecting the whole organ. The pressure in the intervillous space rises. The blood in the intervillous space is driven back into the utero-placental coil arteries which rupture. The spasm passes off and hæmorrhage occurs in the spongy decidua completing the placental separation.

That spasm of the uterine muscle does occur in the initial stages of accidental hæmorrhage is rendered likely by the fact that macroscopic tears of the serosal coat of the uterus are frequently discovered at autopsy and laparotomy.

That the utero-placental vessels tend to dilate and possibly rupture in normal pregnancy has been pointed out by McKelvey (1939) who states that these vessels have lost the usual components of an arterial wall in addition to their endothelial lining.

A series of slides to demonstrate some of the features of the coil arteries in their development and fully developed state were shown.

Glass models of these arteries showing that the convolutions are not only spiral in form but show retrogressions and other meandering forms were exhibited and the effect of such convoluted vessels in reducing pressure was demonstrated by a model.

The slides and models were prepared in the Zoological Laboratory, Trinity College, Dublin, by Mr. Douglas Glen. The expenses were borne by the Medical Research Council of Ireland.

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Section of Ophthalmology

President—Professor W. J. B. RIDDELL, M.D., F.R.F.P.S., F.R.S.Ed.

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The Retinal Vessels in Hypertensive Disease

By ROBERT LEISHMAN, M.D.Glasgow

THERE is a very close association between hypertensive disease and arteriosclerosis. This relationship is early recognized by studying the retinal vessels in hypertensive subjects and may be a source of confusion in fundus interpretation.

Clifford Allbutt (1915) distinguished clinically between senile involutionary (decrescent) sclerosis, and arteriosclerosis secondary to hyperpiesia. Senile involutionary sclerosis is attributed to a progressive replacement fibrosis which occurs with advancing years and independently of hypertension. Hypertensive arteriosclerosis is described by Muir (1951) as a chain of events from arteriolar hypertonus, by way of reactive hypertrophy and hyperplasia, to final replacement fibrosis. Degenerative lesions, fibrinoid degeneration and arteriolar necrosis, may occur in the small arteries and arterioles in established hypertensive disease, and are considered to be the direct result of mechanical strain. Although the ultimate cause of the hypertensive state is still unknown, the presence of hypertension is associated with increased peripheral resistance due to generalized hypertonus of the arterioles. Moreover, it is thought that vascular sclerosis affords some protection to the individual from the more severe manifestations of the condition (Hadfield and Garrod, 1948).

The ophthalmoscope enables us to observe the blood column in a retinal arteriole, since the wall of the vessel is almost transparent in health; and from variations in the appearance of this blood column and its immediate surroundings we may infer the presence of changes, either of physiological or of pathological nature, which occur in the vessel wall. The problem which confronts the oculist is twofold:

(*a*) To recognize the existence of the hypertensive state from the fundal appearances, and

(*b*) To recognize the presence of sclerosis, and to identify its type.

The Central Artery and its Retinal Branches

In structure, there are striking differences between the central artery in the nerve and its branches in the retina. The central artery is a muscular vessel. Its wall consists of well-defined coats or tunics: endothelium, internal elastic lamina, zone of smooth muscle, and a relatively small amount of collagenous adventitia. The retinal artery, however, has no such arrangement into tunics. The endothelium is easily recognized, but there is no continuous internal elastic lamina. The remainder of the wall consists of cells and fibres having no regular arrangement. The cells are of fibroblastic appearance and the fibres are mainly collagenous with an occasional elastic fibre. There is a notable absence of smooth muscle cells in specimens well stained by van Gieson's method. Yet these arteriolar vessels of the retina are known to be capable of constriction and relaxation. Furthermore, it will be seen that the proportion of fibres and nuclei may vary, that the amount of collagen may be excessive, and that many new elastic fibres may appear in the walls of the vessels in certain circumstances.

Senile Involutionary Sclerosis

The manifestations of senile involutionary sclerosis can be recognized with the ophthalmoscope. Ballantyne (1937) has described the features presented by senile retinal vessels in the absence of hypertension. Further evidence of decrescence is also to be found in the fundus background.

The optic disc is slightly pale and hollow. The retina shows disturbance of the hexagonal pigment layer, usually most marked at the temporal margin of the disc and in the macular region; colloid bodies occur in the fundus. The arteries, generally, are narrower, paler, less brilliant, straighter in their course, and branch more acutely than those seen in patients in the prime of life. The veins also are narrow. Fine parallel zones of partial concealment of the vein may be seen on both sides of the arterial blood column at some of the arteriovenous crossings. In the absence of hypertension, congestion of the vein distal to an arteriovenous crossing, indicating impediment to the venous return, has not been observed.

This picture is consistent with average good health in an elderly person, but is sometimes seen in middle-aged people whose general health is failing without obvious reason. Ballantyne points out that "senility is not a mere question of age. Changes commonly regarded as senile may occur in early middle life, and old age may find all or most of them absent". When this picture of senile

involutionary sclerosis is present it is associated with slight elevation of the systolic blood pressure, without corresponding elevation of the diastolic level, giving an increased pulse pressure. Greater force is required to move the same amount of blood into rigid vessels, thus raising the systolic pressure; and, since the resilience of the vessels is defective, the diastolic pressure is not proportionately maintained.

No more advanced changes than these are found in healthy old people.

Senile Involutionary Sclerosis with Hypertension

Certain fundi, while presenting general features of senility, show modifications in the vascular picture which fall into two types:

(a) The retinal arterioles remain pale, straight and narrow in their peripheral portions, but if one of them is followed from the periphery towards the disc, it suddenly becomes more red, wide and tortuous, resembling a normal vessel in a younger age-group. Towards the disc also, segments of unduly wide calibre may be separated by other segments which are of expected calibre (Fig. 1).

(b) In other arteriolar vessels, the main channel is unduly red, wide, and sinuous, well out to the periphery, while branches of the second and third order are pale, straight and narrow (Fig. 2).
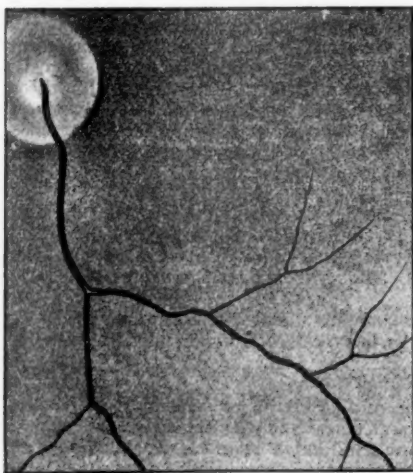
FIG. 1.—Senile involutionary sclerosis with hypertension. Fibrotic portions of the arteriole are dilated; intervening portions are hypertonic.
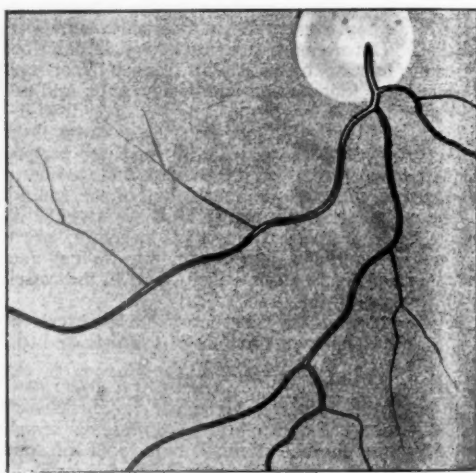
FIG. 2.—Senile involutionary sclerosis with hypertension. The main channel of the vessel is dilated (fibrosis); secondary branches are hypertonic.

There is no increase in the degree of concealment at arteriovenous crossings. The length of vein distal to some of the arteriovenous crossings shows deeper colour, with increased width and tortuosity—congestion of the vein.

Those parts of an arteriole which have become largely fibrous and have lost resilience as a result of senile replacement fibrosis, cannot respond by active vasoconstriction in the hypertensive state. Thus arteriolar hypertonus is limited to the distal parts of the vessel and smaller branches, while fibrotic portions undergo passive stretching and thinning of the wall, with heightening of colour and increase in calibre and length.

These vascular features occur in elderly subjects or in younger persons who show manifestations of premature senility. The great majority of hypertensives seen in the ophthalmic out-patients clinic of a general hospital fall into this group. The patients are often symptom-free and the condition is recognized in the course of routine fundus examination, but sometimes they complain of headaches and giddiness and think they need a change of glasses. Several cases have presented on account of diplopia, with slight paresis of the extra-ocular muscles. The diastolic blood pressure has been found to range from 100–140 mm.Hg, while the systolic levels may reach 250 mm. or more.

It is important to recognize this group of cases, in which the larger retinal arterioles appear red, wide and tortuous even to the point of showing dilatations of calibre in the hypertensive state. Vessels of this description, set in a senile fundus background, give a picture which is consistent with benign hypertension.
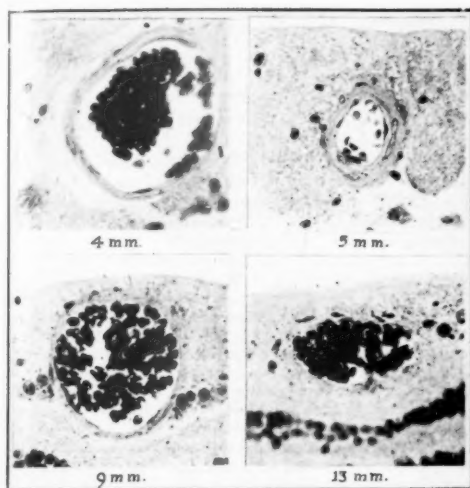
FIG. 3.—Senile involutionary sclerosis with hypertension. Serial transverse sections of the superior temporal retinal artery under uniform magnification (× 300). Distance from disc in mm. Wide lumen, thin wall—relative fibrosis; where lumen is narrow, wall is normal. H. and E.
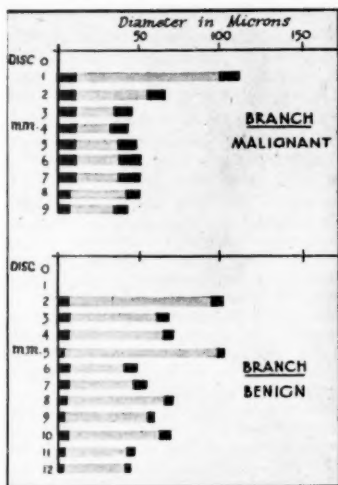
FIG. 4.—Superior temporal retinal arterioles from a case of malignant hypertension and a case of senile involutionary sclerosis with hypertension. Mean width of lumen and wall thickness at intervals of 1 mm. along the vessels.

Pathology of the Senile Hypertensive Group

A planned and detailed study of the histology of the retinal vessels has been carried out in 3 cases in this group. After early fixation of the posterior segment of the eye in formol saline, the retina was removed and examined on the flat. Paraffin sections were cut serially at right angles to the course of the main artery of each quadrant. Adjacent sections were studied in the following stains: Hæmatoxylin and eosin, van Gieson, Weigert's elastica, Mallory's connective tissue stain, Mallory's phosphotungstic acid hæmatoxylin, and Feulgen's reagent.

Illustrative case.—Male, aged 69. Hypertensive for many years. Onset of angina of effort three years before death. B.P. = 160/102. Admitted to hospital with cerebral hæmorrhage. B.P. = 250/105. Died five hours later. Post-mortem findings were those of benign hypertension.

Ocular histology. Arteries in the optic nerve.—The central artery in the optic nerve showed considerable fibrosis and there was a patch of atheroma near the cribriform plate. The smaller arteries in the periphery of the nerve showed two types of change: (*a*) The main vessels in each quadrant were relatively narrow, and there was accentuation of the internal elastic lamina. (*b*) Most other vessels showed senile hyaline change.

The retinal arteries (Fig. 3).—The walls of the retinal arteries never showed an absolute increase in the amount of collagen. Portions of arteries near the disc had a wide circular lumen with thin collagenous walls, in which there was a relative scarcity of cells. The wide circular lumen suggested loss of resilience; the walls were disproportionately fibrous. Such portions were separated by segments of the vessels having more normal calibre, thickness and cell content. The small vessels were of normal appearance. The width of the lumen and the thickness of the wall were estimated in microns from measurements taken at intervals of approximately 1 mm. along the length of a large retinal arteriole (Fig. 4). The wide parts of the lumen were associated with thin relatively collagenous walls; the wall was more normal in appearance where the lumen was narrower.

Hypertension in Children

Ballantyne (1937) has also drawn attention to the differences between the normal fundus appearances in the young and in the aged. With regard to the former he states "the picture as a whole is a more brilliant one, the surface reflexes of the retina are brighter, the arteries and veins have more sinuous curves, their blood columns are darker, and there is a broad and more continuous axial streak, even in the veins".

Transient hypertension, which subsides rapidly with rest, is often, if not always, associated with acute glomerulonephritis in children. I have observed 12 such cases during the past year, and have had fundal drawings made at various stages of the illness in selected cases. The fundi were examined on admission and for a few weeks thereafter. Not all cases were hypertensive, but when hypertension was present it could be distinguished in the fundus by diffuse constriction of the retinal arterioles which appeared unduly pale, straight and narrow. Observation was repeated in these cases until

the vessels regained more normal width and tortuosity. Sometimes this occurred quickly after the fall in blood pressure, but in other cases the vessels remained constricted for some ten days or more. In no case of acute glomerulonephritis with hypertension in children did we observe concealment at an arteriovenous crossing. In the vessels of a child age-fibrosis is minimal.

The straightening and narrowing of the retinal vessels in the hypertensive state is probably a manifestation of arteriolar hypertonus, supported by the fact that the vessels become wider and more sinuous after the fall of blood pressure.

Apart from this type of case, I am informed that hypertension in children is very rare and usually severe. 2 cases were observed concurrently, providing interesting and contrasting features.

Case 1.—Girl, aged 10 years. Pyelonephritis of one kidney. Specimens of urine from the opposite ureter were normal. The diseased kidney was removed and found to have a bifid ureter. Only one branch and its related renal tissue was affected by pyelonephritis. Hypertension persisted after operation and reached high levels (B.P. 260/170), although specimens of urine from the remaining kidney were still normal. Death occurred in terminal uræmia twenty months from the onset of the symptoms. The course of the illness may have been prolonged by treatment with hexamethonium bromide.

The fundus picture was one of malignant hypertension when first seen about one year from the first symptom. The normal brilliance of the child's fundus was lacking. The disc was grossly œdematous, but the œdema was of clear type and did not obscure the small vessels on the disc surface. The retina was pale and slightly œdematous along the course of the main vessels. The arterioles were also pale, although of moderate calibre. They were unduly tortuous, and the loops occupied all three dimensions of the retinal space. Portions of the arteriolar walls were thickened and less translucent—hyperplasia of the wall. The blood column was even more pale, narrowed and finely variable in calibre, and there were broad parallel zones of concealment of the veins at arteriovenous crossings. The veins distal to such crossings were congested. The arterial branchings were wide-angled and back-curving. Small, round, hard-edged, white exudates were numerous around the disc and in the central area where there was a well-defined macular star. Many small hæmorrhages were present and were seen to come and go during a year's observation.

Case 2.—Girl, aged 10 years. Pyelonephritis was bilateral from the beginning. Death occurred in uræmia three years later. Hypertension was severe for her age but the diastolic level was never over 115 mm.Hg.

The fundus picture was drawn about one year before death and is therefore comparable with the previous case. It presents certain contrasting features; there was gross œdema of the disc and some œdema of the retina, but in this case the œdema was of a cloudy nature. Hard-edged, white exudates were minimal but patches of opaque "cotton-wool" exudate were frequent. In many places the vascular pattern was obscured by œdema or exudate which prevented consecutive study of a vessel along its length. Proximal portions of the artery, however, were seen to be red and wide (fibrosis) other portions in the mid-zone were pale, narrow, finely variable in calibre, and showed broad bands of concealment at crossings (hyperplasia), while distal portions were straight and narrow (hypertonus). Veins were congested distal to arteriovenous crossings.

The first case must be regarded as one of malignant hypertension of renal origin in which renal failure was but a brief terminal episode. In the second case renal disease dominated the picture and was complicated by severe hypertension.

Hypertension in Adults of Intermediate Age

The clinical features of the normal adult fundus vary between the extremes met with in children and in the aged. The retinal arteries have an expected colour and width of calibre, and usually run a moderately sinuous course.

When hypertension occurs in people of the intermediate age-group, three main types of fundus picture can be recognized:

(1) Early hypertension with arteriolar hypertonus.
(2) Malignant hypertension with arteriolar hyperplasia and hypertonus.
(3) Established hypertension with secondary arteriosclerosis; fibrosis, hyperplasia, and hypertonus affect different parts of the same retinal arteriole.

Early Hypertension with Hypertonus

The fundus background has none of the appearances associated with senility. The retinal arteries are slightly pale, and in general, straight and diffusely narrow, even up to the margin of the disc and on to its surface. There is absence of concealment at arteriovenous crossings but congestion of the distal portion of the vein is seen at some of the crossings. This picture is interpreted as hypertonus in young resilient vessels, associated with a raised level of diastolic blood pressure. Such cases are not uncommon.

Malignant Hypertension with Hyperplasia and Hypertonus

Although it is probable that a phase of simple hypertonus occurs as a brief prelude to the development of malignant hypertension in the retina, this transition has not yet been observed.

In malignant cases (Fig. 5), papillœdema and retinopathy are usually present when the fundus is first examined. The papillœdema is always well marked and is often associated with œdema of the retina. The clarity of the œdema fluid suggests that it is a transudate, and this is in contrast to the appearance in cases of renal retinopathy. Although the arteries may approximate to normal calibre near the disc, they are always pale. In some portions the pallor is accentuated and the calibre of the blood column is reduced and finely variable. Broad parallel zones of concealment are seen at many

of the arteriovenous crossings, and are more marked if that segment of the artery shows increased pallor. These appearances suggest arteriolar walls, which are thickened but not necessarily rigid. They will be shown to be associated with hyperplasia of the supporting elements in the vessel walls.

The arteries are tortuous in three dimensions; the small vessels may be straight and narrow, suggesting hypertonus.

This picture is associated with very high levels of diastolic pressure ranging up to 170 mm.Hg. Sudden visual deterioration is often the presenting symptom, and the patient is usually unaware of the gravity of his condition. Fortunately such cases are rare.

Pathology in Malignant Cases

The pathological findings in this group are drawn from 3 cases and the same systematic method of examination was followed.

Illustrative case.—Spinster, aged 50. Symptoms for two years. Five months before death B.P. = 260/150. A few hours before death B.P. = 260/170. Massive pericardial effusion. Died of heart failure. Post-mortem findings were those of malignant hypertension.

Ocular histology. Arteries in the optic nerve.—There was no evidence of fibrosis in the central artery. The lumen was wide and circular and the internal elastic layer reduplicated. The main feature was a swollen zone of smooth muscle in which individual elements lacked definition in all the stains. Smaller arteries in the periphery of the nerve showed well-marked hyperplasia of the internal elastic lamina.

The retinal arteries (Figs. 4 and 6).—The retinal arteries had thickened hazy walls in all stains. The wall-cells were not increased in number. Their nuclei were often much swollen and stained faintly; occasionally a nucleus was fragmented. Many new elastic fibres were scattered irregularly at all levels in the wall. The lumen was never excessively wide. In the smaller vessels it was diminished where the walls were most thickened, but was usually circular. Precapillary vessels were often markedly dilated with greatly swollen endothelial cells.

These changes suggest hyperplasia of elastic elements in the walls with cloudy swelling of all elements. They do not necessarily imply loss of resilience, but are probably associated with diminished translucency.

In addition to these general features in the vessels, focal lesions were observed in several arteries. The lesions were of the following types:

(1) Necrotic foci in the swollen wall.
(2) Acute arteriolar necrosis with dissecting hæmorrhage.
(3) Intra-arteriolar thrombosis in relation to a focus of degeneration in the wall.

Arteriolar Thrombosis

Arteriolar thrombosis occurs in severe cases of hypertension and can be recognized by the ophthalmoscope (Fig. 5). A mass of hæmorrhage appears around a retinal arteriole. The vessel is invisible distal to the hæmorrhage, but the proximal part is nearly filled with blood which is of deeper colour than arterial blood—thrombosis. The thrombotic blood column extends proximally almost to the parent artery, or, in other cases, to the nearest branch or branches, which are also thrombotic.
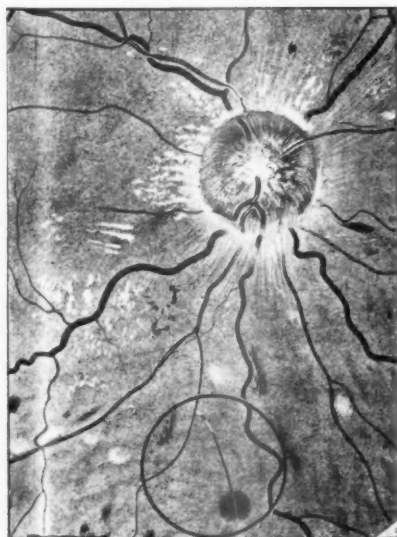

FIG. 5.—Fundus in malignant hypertension showing thrombosis of an arteriolar branch of the inferior temporal artery.
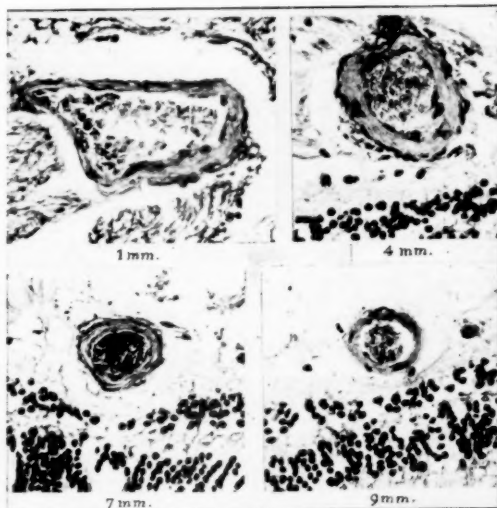
FIG. 6.—Malignant hypertension. Serial transverse sections of the superior temporal retinal artery under uniform magnification ($\times$ 300). Distance from disc in mm. Swollen hazy walls. Lumen never excessively wide. H. and E.

The vessel then continues as a pale white ribbon until it joins the parent vessel. In the course of a few weeks the hæmorrhage becomes slightly larger and less dense, and the vessel may be seen within it as a white ribbon; the thrombotic blood column disappears, the distal portion first.

This picture probably results from focal necrosis in the arteriolar wall, and may well be associated with similar lesions in the vessels of other organs. It has been seen in severe cases of benign hypertension as well as in malignant cases.

Established Hypertension with Secondary Arteriosclerosis

Apart from the early cases with generalized constriction of the retinal arterioles, and occasional cases showing malignant features, the great majority of middle-aged people with hypertension fall into a group presenting mixed changes in the same retinal arteriole (Fig. 7). There is no evidence of
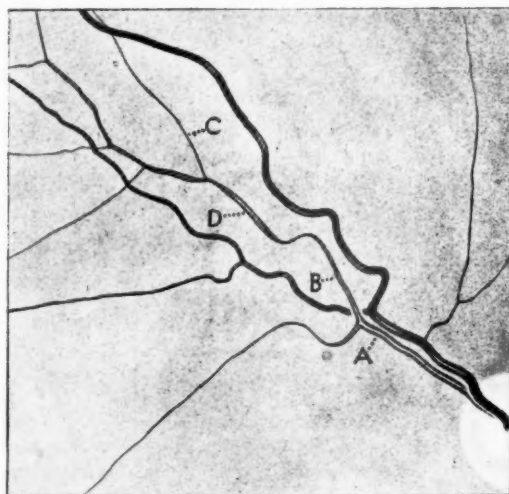
FIG. 7.—Hypertension with secondary arteriosclerosis. The main stem shows fibrosis (A and D) and hyperplasia (B); secondary branches are hypertonic (C) (× 300).

senile changes in the fundus. The disc is normal, or has a pale white haze which does not obscure the vessels on its surface; there is no gross papilloedema. The arterioles are, in general, more tortuous than normal and there is impediment to venous outflow at arteriovenous crossings so that the veins appear full and tortuous distal to the crossings. Small arterioles, and the more distal portions of the larger ones, are straight and narrow—hypertonus. In the mid-zone the arterioles are bounded by broad parallel zones of concealment of the veins at arteriovenous crossings and in these thick-walled portions the colour of the blood column is pale and its width finely variable but narrow—hyperplasia of the arteriolar wall. Nearer to the disc the arterioles appear red and full—fibrosis.

Such cases cannot be classified as malignant hypertension, and yet are more severe than the elderly fibrotic type. These patients usually give a history of four or more years' duration and often the hypertension proves to have originated in chronic nephritis or toxæmia of pregnancy. In hypertensive subjects, if given time, healthy vessels go through a process of adaptation from physiological hyper-tonus, by way of hypertrophy and hyperplasia, to partial replacement fibrosis. The appearance of these vascular changes of mixed type in the same retinal arteriole in hypertensive cases is consistent with moderately severe elevations of the diastolic pressure (110–140 mm.Hg). This fundus picture is associated with a severe degree of benign hypertension, and usually occurs in young and middle-aged people.

Pathology in Hypertension with Secondary Arteriosclerosis

It has only been possible to examine one case in this group, but the findings are worth recording.

Case.—Male, aged 74. The left kidney was removed twenty-five years ago because of pyelonephritis. He was found to have hypertension eighteen months ago. B.P. = 270/130. The right eye was excised on account of a malignant melanoma situated nasal to the disc. Fibrotic, hyperplastic, and hypertonic portions of the superior temporal artery were labelled on a fundus sketch before excision of the eye. The retina was photo-graphed on the flat. Histological techniques already described were used to study the material.

Histology of the Retinal Vessels (Fig. 8)

Fibrotic portion.—The lumen was wide and the wall relatively thin. There was a relative diminution in the number of wall cells. The wall was mainly collagenous, although a few elastic fibres were distributed throughout the collagen.

Hyperplastic portion.—The lumen was more narrow and circular, and the wall thicker. The wall contained many new elastic fibres amongst the collagen; a considerable number of nuclei were present. All the elements stained fairly well, in contrast to the appearance in malignant cases with hyperplasia of the supporting elements.

Hypertonic portion.—The lumen was fairly narrow and circular. The wall was of normal thickness and relatively cellular. A few elastic fibres were present even in relatively small vessels.
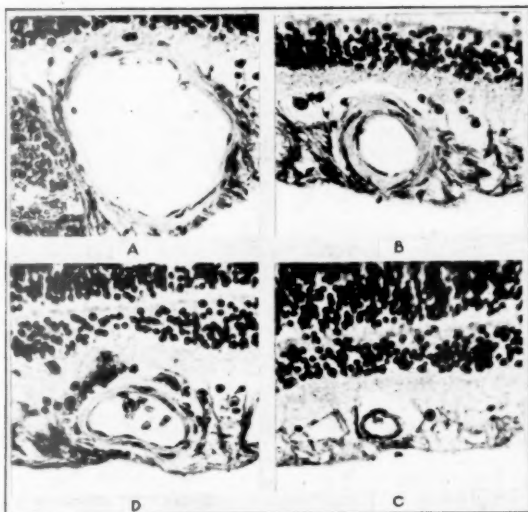
FIG. 8.—Transverse sections of vessel shown in Fig. 7. Relative fibrosis (A and D), hyperplasia (B), and normal wall (C) (× 300).

SUMMARY

Decrescence appears slowly in the arteries with advancing years, but may be accelerated by social, economic or hereditary factors.

Cases of hypertensive disease may be classified by the fundal appearances into two main groups:

A. Those associated with retinal evidence of senile or premature involutionary sclerosis.

B. Those without such features.

In the first group, hypertension occurs in vessels already largely defended by involutionary sclerosis. The fibrotic portion of the vessel stretches with increased pressure, while vasoconstriction is limited to peripheral vessels and smaller branches. This picture is associated with true benign hypertension.

In the second group, early cases show arteriolar hypertonus in the retina. A few cases present with advanced retinopathy of malignant type; the vessels have the features of hyperplastic change and hypertonus. Many other cases, however, present a fundal picture in which fibrosis, hyperplasia, and hypertonus are found in different portions of a single retinal artery and its branches. Although these latter cases are more rapid in development than the elderly decrescent type, they do not run the course of malignant hypertension; the fundus appearances are associated with a severe degree of benign hypertension and the arteriosclerosis is of reactive or secondary type.

The retinal manifestations of hypertensive disease are of frequent occurrence, and can often be detected initially with the ophthalmoscope before the patient has had occasion to consult a physician. The retinal arteries may be well defended by senile involutionary sclerosis. They may be young and plastic, and therefore more liable to suffer acute degenerative changes; or they may show evidence of secondary reactive sclerosis when the hypertension is of slower evolution. These various types of sclerosis can be recognized in the fundus and this may be the only direct evidence of the state of the arteries available to the physician or the vascular surgeon.

This work was carried out at the Tennent Institute of Ophthalmology, and in the wards of the Western Infirmary, and the Royal Hospital for Sick Children, Glasgow.

My thanks are due to Professor W. J. B. Riddell and my colleagues in these hospitals; also to Mr. Gabriel Donald, medical artist, and Mr. John Watt, senior technician. My thanks also to Professor A. Loewenstein for allowing me to refer to his collections of histological specimens.

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Mr. R. U. Gillan, speaking as one who had at one stage been particularly interested in the subject of Mr. Leishman's paper, said he had followed up a series of cases from the prognostic point of view, to ascertain whether there was any correlation between the vascular appearances and the prognosis as regards duration of life. It had appeared that there was a considerable degree of correlation: the more degenerative the vessels the worse the prognosis. The most significant feature was the calibre of the vessels, the most serious significance of all being associated with the most finely attenuated vessels. These appeared to carry the worst prognosis. On the other hand, strangely enough, the hæmorrhages and exudates which were associated with such cases did not appear to carry the same prognostic significance. There was a definite correlation between the changes in the vessels and the blood pressure. Both were important from the point of view of assessing the prognosis, but of the two, it appeared that the changes in the vessel walls were the more significant. It seemed a pity that insurance societies did not employ ophthalmologists, rather than general physicians to assess such cases.

Dr. L. B. Somerville-Large had been carrying out work for a number of years on the toxæmias of pregnancy. He found that in such cases there was a high blood pressure over a comparatively short period of time, and change in the retinal vessels. He used the phrase "angiospasm" frequently when talking to post-graduates and in papers he had written on the subject. He would have used the phrase "local angiospasm" in regard to a number of the illustrations shown during Dr. Leishman's Address. Was he right in assuming that the correct description would be a thickness of the wall of the vessel which produced the narrow lumen shown, rather than an angiospasm? Personally he had never seen a single angiospasm in a toxæmia of pregnancy disappear, though he was aware that it had been described. He had been examining these cases for nearly twenty years and was about to undertake a much wider survey. If the toxæmias of pregnancy fell into the group so beautifully shown in this paper, where the vessel wall was thickened, then one would not expect the vessel wall to become normal. The vessel would go throughout life with what appeared to the speaker to be a local angiospasm.

Perhaps the strangest factor in the toxæmia of pregnancy was that the individual could have a severe eclampsia with enormously high blood pressure and no change whatever in the vessel walls, whereas there could be a comparatively low, though sustained, blood pressure and gross changes in the vessel walls.

The speaker added that he was extremely sorry that in the particular case in which sections of the arteries were made and one saw the retina when removed and before the sections were cut, none of them went through the part he personally would have been particularly interested to see, namely, the narrow area of the vessel. All the sections had been taken through the thicker areas; none through what he would regard as being the angiospasm.

Dr. Robert Leishman in reply said that on the prognosis attached to attenuated vessels extreme hypertonic attenuation indicated young healthy arterioles and, in general, the more severe manifestations of hypertensive disease were likely to develop in such vessels.

In reply to Mr. Somerville-Large, he had carefully avoided the use of the term "angiospasm" in favour of the conception of arteriolar tonus. Spasm seemed to imply a sudden and transitory vasoconstriction, and its use had recently been deprecated when applied to a condition of sustained hypertonus in a vessel wall. It was significant that, in the course of examining many fundi day by day, constricted portions of vessels were not seen to relax suddenly during observation, although this would be expected to occur if the constriction were spastic. The term "angiospasm" might properly be applied to a local region of constriction in a retinal vessel at the site of an embolus. He could recall a case in which such a local constriction had disappeared by the third day, but the actual relaxation of the spasm had not been observed.

Hypertension occurring in pregnancy was of particular interest because the hypertensive state might undergo reversion with termination of pregnancy. The pregnancy group might also include two types of case, differing according to whether hypertension developed early or late in pregnancy.

With regard to the histological appearance of the small arterioles, the walls were often normal suggesting a correlation with hypertonus in life. In malignant cases the capillary vessels were often markedly dilated with swollen endothelial nuclei.

Asked whether one of the young patients had shown relaxation of the constriction of the arterial wall, Mr. Leishman said that this was so. Vessels which were straight and narrow one day showed an appreciable difference in width and tortuosity on the following day, but the actual transition from one state to the other was not observed in any case during examination. The relaxation of hypertonic vessels in cases with high blood pressure did not seem to be a sudden process.

The President thought that more information would come from Dr. Leishman's work because of the intention to follow up the cases and observe their movement from one category into another; this analysis would form the essential proof of the thesis put forward. It would be a long-term project but already a large amount of interesting material had been collected.

Section of Anæsthetics

President—JOHN GILLIES, C.V.O., M.C., M.R.C.P.Ed., F.R.C.S.Ed., F.F.A. R.C.S.

[*March 7, 1952*]

THE PRESENT POSITION OF ANÆSTHESIA FOR NEUROSURGERY

Dr. A. R. Hunter (Royal Infirmary, Manchester): *The Theoretical Basis of Neurosurgical Anæsthesia.*
  There is a belief in some quarters that anæsthesia for neurosurgical operations involves merely patience and the use of non-toxic agents. While this is possibly true of a small number of procedures, particularly of the extracranial type, it is equally true that in every neurosurgical clinic operations are begun which cannot be completed for reasons directly traceable to the activities of the anæsthetist and that the number of such cases varies inversely with his skill. It is also true that during certain types of operation there appear warning signs of impending danger which only the anæsthetist can detect. If he fails to make the necessary observations or to interpret them aright it may well be that an operation, which from the surgeon's point of view was technically perfect, results in the death of the patient in the post-operative period as a consequence of a disturbance whose early signs were apparent during the operation. The responsibility of the neurosurgical anæsthetist is thus far heavier than that of his colleagues in other fields. Not only must he find anæsthetic methods which are safe and satisfactory when used over long periods and when the explosion risk is present, he must also be certain that he does nothing which will in any way hamper the surgeon, e.g. by increasing the bulk of the brain or the intracranial venous pressure. Finally he must be prepared to make appropriate observations of changes in the vital functions and to interpret them in terms of the effect of the anæsthetic, of blood loss or of damage to the brain, brought about by the disease, the investigations which have been used to elucidate it, or the manipulations of the surgeon.

Anæsthesia and the Intracranial Pressure

  The physiological and pharmacological problems associated with prolonged anæsthesia with non-explosive agents have by now been satisfactorily solved and, in fact, are of comparatively little importance in relation to neurosurgical anæsthesia. The basic consideration in such cases is the effect of the method employed on the intracranial tension. White and others (1942) measured in the experimental animal the effect of various factors on the brain bulk and found that the type of anæsthetic agent was of relatively minor significance. By far the greatest changes were those caused by oxygen lack or carbon dioxide retention. Every possible step should therefore be taken to avoid these complications both in the induction period and during the maintenance of the anæsthetic, since the disturbance of brain volume caused by even very short periods of anoxia or carbon dioxide accumulation persists for at least half an hour afterwards, and may last for the entire duration of the operation. Smooth induction of anæsthesia without spasms of coughing is thus absolutely essential, and intubation should be carried out either with the aid of topical anæsthesia or of a relaxant. If this latter type of drug is used an early return of normal breathing should be aimed at. Further, the greatest care should be taken to produce artificial ventilation at least equal to the expected spontaneous respiratory excursion as long as the paralysing effect of the drug is present.

  Special precautions to prevent anoxia and carbon dioxide accumulation are equally necessary during the maintenance of anæsthesia. Again spasms of breath-holding, coughing or bucking on the endotracheal tube must be prevented at all costs. Further, as Molyneux and Pask (1951) have pointed out, unless very large flows are used some degree of rebreathing and therefore carbon dioxide accumulation is very likely with the ordinary Boyle's apparatus. It is thus well worth while using a non-return valve in the hose, if one of a pattern which does not load respiration is available and the machine will deliver an appropriately large gas-flow of known composition.

Anæsthesia and the Venous Pressure

  Control of the intracranial venous pressure is another matter of major importance during neurosurgical operations. The veins of the brain are exceedingly thin-walled, as they derive much of their

support from the water-bed of the cerebrospinal fluid contained within the rigid skull. After this support has been withdrawn by craniotomy they become particularly vulnerable to rupture by surges of pressure within them. Once again, therefore, it is necessary to take special care that the intracranial tension is not suddenly raised during the operation by a spasm of coughing. Even in the absence of frank coughing, some patients have, as Brennan (1938) has pointed out, a type of breathing in which expiration is forced and is associated with a sharp contraction of the flat muscles of the abdomen. In such cases not only is the general level of venous pressure high but each expiration causes a temporary further increase. This high pressure inevitably leads to increased bleeding in the operation site. Further, it makes it doubly likely that if by some mischance the patient does cough, one of the cerebral veins will be ruptured. If this should happen in the brain-stem the accident will almost certainly have a fatal outcome. The remedy for this trouble is adequate topical anæsthesia of the trachea, the use of an endotracheal tube which passes in adults no more than two inches beyond the vocal cords and whose end is thus unlikely to impinge on the very sensitive carina, together with a depth of general anæsthesia sufficient to extinguish what is left of the patient's cough reflex.

The Sitting Position

For many neurosurgical operations the sitting position is preferred by the surgeon and in such cases the pressure in the intracranial veins may be subatmospheric throughout the entire operation. Veins which are opened will therefore not bleed and their presence may go altogether unobserved until signs of air embolism make their unwelcome appearance. The vessels from which this danger is most likely to arise are those of the suboccipital venous plexus and the mastoid emissaries; a small tear in the lateral sinus produced during the bone removal is another likely source of trouble. There are three ways in which the anæsthetist can help to avoid such accidents, which are by no means always fatal. First, smooth anæsthesia and avoidance of over-breathing or coughing should prevent any serious intake of air. Secondly, while this danger is present he can ensure that any open vein is found early, by compressing the jugular veins in the neck at regular intervals. Lastly, he should regard any unexplained change in blood pressure and pulse-rate occurring in a patient in the sitting position as a warning that a small embolism has already occurred, until the converse is proved by the absence of venous bleeding on jugular compression.

The sitting position brings with it other dangers besides air embolism. As would be expected some degree of vasomotor instability develops but on the whole actual fall in blood pressure is surprisingly rare once the circulation has become stabilized. Hypotension may, however, develop particularly in patients with serious lesions in the posterior fossa, during the first half-hour or so after they have been sat up. In a few cases only laying the patient down will restore the circulatory stability but usually the intravenous injection of a small dose of a vasopressor will serve to stabilise matters. Not only so, once stability has been attained patients will withstand depressor influences remarkably well. Tolerance to blood loss will be almost as great as in the horizontal position and the fall in blood pressure which is produced by dissecting the lower pole of an acoustic neuroma from the vagal rootlets will right itself spontaneously.

Patients in the sitting position, on the other hand, are very sensitive to the depressor action of drugs. The dangers of hexamethonium in such cases are self-evident but it is less well known that pethidine has a profound depressor effect on those in the erect position and a colleague had a very serious, though not fatal, accident traceable to its use. Small doses of the ultra-short-acting barbiturates can be used to supplement nitrous oxide and oxygen in patients in the sitting position but the amounts injected must not exceed 3 c.c. of 5% thiopentone, and they must be injected slowly if accidents are to be avoided. Even with care there will very occasionally occur a fall in blood pressure. Fortunately this hypotension is also amenable to treatment with a vasopressor.

The Prone Position

The prone posture used for laminectomy and some types of sub-occipital approach to the cerebellum brings with it a group of special problems. The necessity for raising the shoulders on rests or sandbags and of allowing the abdomen freedom to expand in inspiration is well known. It is, however, far from well known that any form of pressure on the anterior abdominal wall, such as may be employed to undo the lumbar lordosis to facilitate access to a prolapsed intervertebral disc, will compress the inferior vena cava against the bodies of the lumbar vertebræ which at this level project far forwards into the abdominal cavity. Pressure on the lower chest will have the same effect. After the caval circulation has been cut off for some time there will appear a steadily increasing pulse-rate and a falling blood pressure which will disappear if the anæsthesia is lightened sufficiently to allow return of the tone of the muscles of the abdominal wall (Hunter, 1950). At this level of anæsthesia, however, the patient will always react to the handling of posterior roots. It is therefore important that it should be realized that any improvement in access to the lumbar spine which is attained by mid-line pressure on the anterior abdominal wall is obtained at the expense of quite a serious circulatory upset. Further, since the vertebral extradural venous plexus forms a large part of the anastomotic circulation for an obstructed vena cava, the apparent improvement in access will be obtained only at the expense of a great increase in the bleeding in the operation field.
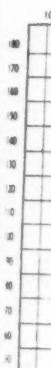
Disturbances of Vital Functions

Though it is generally known that morphine should not be used as premedication in neurosurgical cases it has not been sufficiently emphasized that all anæsthetic agents are potential inhibitors of breathing in those whose intracranial pressure is very high. Indeed in one patient with an extradural posterior fossa clot who had just been resuscitated from apnœa by ventricular tap, the addition to the oxygen which was being given, of some 60% nitrous oxide, to control restlessness, was followed by respiratory failure. This patient's story has been reported in detail by Jefferson and Johnson (1950). It is probable that the apnœa which is liable to develop when unnecessarily deep anæsthesia is used in those with tumours affecting the centres controlling respiration, has a similar origin, though Brennan (1938) has suggested that the rise in intracranial pressure which occurs when ether is added to nitrous oxide and oxygen is also of importance in this respect. The main point which emerges from this is, however, the necessity for using the lightest possible anæsthesia which will be safe in patients with high intracranial pressures. It is also important to note that nitrous oxide, oxygen and ether anæsthesia is not so innocuous as Harvey Cushing found open ether to be.

In addition to his duties of providing unconsciousness for the patient, immobility for the surgeon and an adequate amount of fluid replacement, the neurosurgical anæsthetist must be prepared to give the surgeon sufficient warning of the development of any major upset of the vital functions of neurological origin. For ordinary working purposes the vital centres of clinical importance are those in the floor of the third ventricle and hypothalamus and those in the floor of the fourth ventricle in the dorsal portion of the pons and medulla oblongata. The hypothalamic centres may be damaged in the course of the removal of the pituitary tumours, Rathke cysts, and suprasellar and olfactory groove meningiomas and the usual sign that all is not well is a fall in blood pressure (Fig. 1).
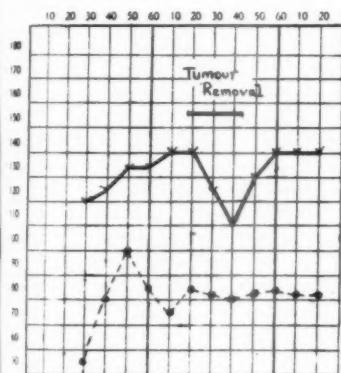
FIG. 1.—Operation chart showing blood pressure fall of hypothalamic origin during removal of pituitary tumour. Blood pressure: solid line. Pulse-rate: broken line.
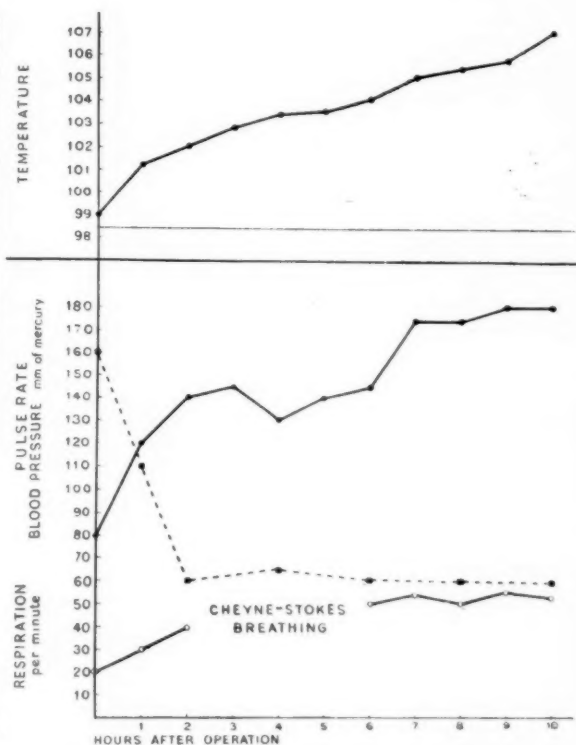
FIG. 2.—Post-operative chart showing rise of temperature, pulse and respiratory rates (solid line) and fall of blood pressure (broken line) after removal of pituitary tumour. (Hypothalamic Disturbance.)→

If this is evanescent or ceases immediately when the surgeon desists from the operation there is some chance that recovery may take place. If, however, the hypotension is severe or prolonged, or if, after recovery the pulse and respiratory rate start to rise (Fig. 2), the outlook is wellnigh hopeless and the patient will almost certainly fail to recover consciousness and die in hyperpyrexia shortly afterwards.

Since a proportion of these disturbances are reversible if they are detected early, it is of extreme importance that the surgeon should at once be notified of any unexplained decline in blood pressure which occurs during any operation in the neighbourhood of the hypothalamus.

The second syndrome of danger during neurosurgical operations signifies encroachment on the vital centres in the floor of the fourth ventricle. It may arise as the direct result of an attempt to remove a tumour which has already invaded these areas of the brain or it may appear because the manipulations of the surgeon are producing rotation of the brain-stem and thus a temporary mild asphyxia of it. The only constant element in this syndrome is a bradycardia. If the cause is a lesion of the vital centres or their immediate neighbourhood bradypnœa or even apnœa will often occur (Fig. 3). If the cause is rotation of a normal brain-stem some rise in blood pressure is a more likely
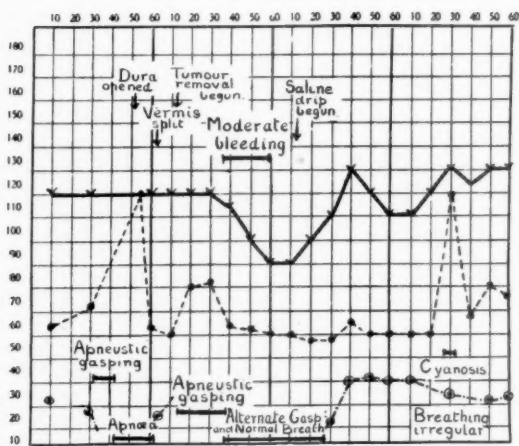
Fig. 3.—Operation chart showing bradycardia and respiratory disturbances during removal of tumour invading the floor of IV ventricle. Uppermost curve: blood pressure. Middle curve: pulse-rate. Lower curve: respiratory rate.
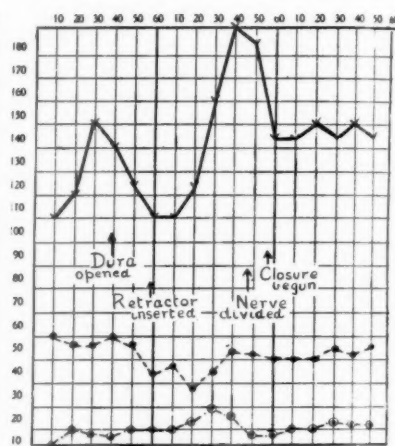
Fig. 4.—Operation chart showing bradycardia and hypertension during rotation of the brain-stem to expose VIII cranial nerve. Uppermost curve: blood pressure. Middle curve: pulse-rate. Lower curve: respiratory rate. The rectangles at the foot of the figure indicate periods of administration of trichlorethylene.

accompaniment (Fig. 4), but the classical syndrome of hypertension, bradycardia and bradypnœa described by Cushing as response to medullary coning is almost unknown on the neurosurgical operating table. It can, however, develop in the post-operative period in patients with a rapidly rising pressure in the posterior cranial fossa. Another variant of this syndrome has been seen in patients in whom subarachnoid bleeding has occurred during the anæsthesia. In them hypertension without bradycardia or bradypnœa may occur.

The third and by far the commonest disturbance of the vital centres to appear in neurosurgical patients is a rising pulse-rate and, apart from the tachycardia of children who have bled unduly, this sign means a rise in the intracranial pressure. It appears most commonly of all in the patient with a cerebellar tumour but it can also be produced by any lesion obstructing the cerebrospinal fluid pathways, no matter at what level. Thus it can be seen in cases of stricture of the iter of Sylvius and in obstructive lesions of the third ventricle. It also develops when a rapidly increasing intracranial pressure in one supratentorial compartment thrusts the tip of the temporal lobe through the incisura tentorii, though the exact mechanism of production of the tachycardia in such cases is not quite clear. Whatever the cause of the tachycardia its significance is always the same, namely an excessive intracranial pressure, and the remedy is to lower this pressure by tapping the lateral ventricle. In the patient with a supratentorial lesion it is usually desirable to proceed at once with an operation to lower the intracranial pressure more permanently and if the patient will tolerate it local anæsthesia should be used alone. Indeed one need have little hesitation in inflicting what appears at the time to be quite a lot of pain on such patients as they will most certainly have a memory blank extending for some time after the episode of compression.

It is mainly with tachycardia developing in patients with cerebellar tumours that the anæsthetist is concerned. The first point to be noted is that in patients with lesions of this kind the induction of ordinary semi-closed anæsthesia is associated with a rise in intracranial tension (Fig. 5). If the result
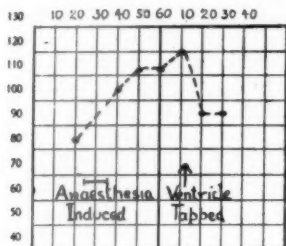
FIG. 5.—The rise in pulse-rate which follows induction of anæsthesia in a patient with a high intracranial pressure and its reversal by ventricular tap.

is tachycardia of 140 per minute or more in an adult or 160 or more in a child the surgeon should be asked to tap the ventricle as a first stage in the operation. If the patient has had an air ventriculogram immediately before the operation it is much wiser to assume that the intracranial pressure already approximates to dangerous levels and the pulse-rate will often indicate that this is so. In such cases it is most desirable that the ventricular tap should precede the induction of anæsthesia. Further, it should not be assumed that because this has been done the intraventricular pressure will remain at safe levels for the rest of the operation. Indeed it is far more likely that it will build up rapidly again and that the pulse-rate will indicate very shortly after induction that ventricular tap is again necessary and it may be necessary again before the operation has reached the stage at which the obstruction to the cerebrospinal fluid flow is relieved.

I should like to acknowledge the help and encouragement which I constantly receive in this sphere of my work from Sir Geoffrey Jefferson and the other members of the neurosurgical team in Manchester.

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Dr. A. J. H. Hewer (Middlesex Hospital, London): *The Practical Aspects of Neurosurgical Anæsthesia.*

To illustrate the practical aspects of the points which Dr. Hunter has dealt with I have chosen five main headings, the first three of which are very closely connected.

   I. The vascularity of the scalp and brain.

  II. The susceptibility of damaged vital centres to anoxia and CO_2 excess.

 III. The possible danger of disturbance of the intracranial dynamic equilibrium resulting from the anæsthetic or from opening the dura when the intracranial tension is raised.

 IV. The danger of direct or indirect surgical trauma to the vital centres, especially during the removal of large intracranial tumours.

  V. The possibility of intracranial hæmorrhage occurring after the bone flap has been replaced, and the importance of diagnosing this complication before the patient leaves the operating theatre.

Every one of these points is important, and each is to some extent under the control of the anæsthetist.

I. Vascularity of the Scalp and Brain

The normal vascularity of the scalp may be exaggerated by factors which cause an increased intracranial tension and thereby an obstruction to the venous exit at the base of the skull. It is seldom more than a nuisance, however, and can be controlled by hæmostatic clips. Vascularity of the brain is not so simple to deal with, since the cerebral vessels cannot always be clamped off without running the risk of ischæmia to some vital part of the brain. Any measure, therefore, which will help to reduce the venous pressure within the skull and simultaneously the intracranial tension, is of vital importance as it may decide the operability of the case. Although arterial bleeding is also important, it is usually the venous bleeding which is most troublesome, since it produces a generalized ooze which is very difficult to stop. If we are dealing with a hæmangioma or an aneurysm then the arterial pressure becomes more important. Since the advent of the hexamethonium compounds we have become more aware of the importance of posture in relieving venous congestion; and, it seems often to be

the posture more than the C6 which is instrumental in producing a quiet operating field. Whenever possible, therefore, we should employ the aid of gravity by having the operation site higher than the rest of the body.

Besides mechanical obstruction and bad posture, a potent cause of venous congestion is respiratory back-pressure, produced by an overloaded expiratory valve or by any resistance in the anæsthetic circuit, or, as is most likely to happen during the induction of anæsthesia, by coughing and straining. Once the pressure inside the skull has been raised, the brain can act as a kind of ball-valve, pressing down on the jugular bulb and maintaining the congestion until active measures, such as ventricular tapping, are taken to relieve the vicious circle. This is well illustrated where the patient has a ventriculostomy, thin polythene tubes being inserted into one or both lateral ventricles to allow C.S.F. to drain into a sealed flask. Any rise in intracranial tension will increase the rate of drainage from the ventricles.

Continuous lumbar puncture pressure readings taken during the anæsthesia are most helpful as an index of any change in equilibrium. When the anæsthetic becomes too light or if the patient begins to strain, there is an immediate and often dramatic rise in the L.P. pressure. The reason for this is twofold: First, there is an immediate rise due to the intra-abdominal pressure created by the use of the abdominal muscles in expiration. This pressure is transmitted from the inferior vena cava via the lumbar veins to the veins of the peridural plexus. These become engorged, and so increase the pressure within the vertebral canal.

Second, then follows a secondary and slower rise due to the venous back-pressure which is transmitted from the mediastinum via the jugulars to the brain, and so to the lumbar cerebrospinal fluid.

II. Anoxia and CO_2 Excess

These will produce the same effect in the brain and in the spine by dilating the capillaries, paralysing the smooth muscle of the veins and causing myocardial asphyxia. This is the picture of congestive failure, and it may lead to a vicious circle by starving an already overtaxed and tired respiratory centre together with other vital centres. If these centres are the victims of disease or of mechanical pressure, they will be unusually susceptible to anoxia. We must adopt, therefore, some reasonably certain method of inducing anæsthesia and of intubating without coughing or straining.

The first essential is adequate premedication. Since the usual drugs like the opiates, pethidine, hyoscine hydrobromide and the long-acting barbiturates are also respiratory depressants, in cases of high intracranial tension they may lead to complications resulting from the hypersensitivity of the disorganized vital centres. Even where the C.S.F. pressure is normal, there is a case against long-acting central nervous depressants, because it is a great help if the patient regains consciousness on the table at the end of the operation. We can then accurately assess the condition of his higher centres and can observe his progress, eliminating the possibility of post-operative hæmatoma formation, which would probably manifest itself by increasing hemiplegia or loss of consciousness and which would have to be dealt with on the spot by reopening the bone flap and excavating the clot. Should the patient fail to recover at the expected rate, it is easier for the anæsthetist to say: "This patient should now be conscious, his coma is not due to the anæsthetic." The uncertainty of action of many drugs used as premedicants does not always make this possible, and they are best avoided.

However, a heavy premedication is by no means absolutely essential for a smooth induction. Much can be done with sensible patients by pre-operative reassurance and explanation by the anæsthetist. When he comes to the theatre the patient should feel he has a friend in court. There is a great need for more of this pre-operative psychotherapy or "pep-talk", as it can have a profound effect on a patient's behaviour under anæsthesia, even though he be deeply unconscious.

For induction we have a choice of methods:

(a) A long and gradual induction with say, thiopentone, nitrous oxide, oxygen and either trichlorethylene or chloroform. I would suggest the avoidance of ether whenever possible, since it produces quite a marked cerebral vasodilatation. Anæsthesia can be reinforced with careful cocainization of the throat and larynx under direct laryngoscopy when the patient is deep enough, and this followed by intubation. This is the orthodox, and probably the safest method.

(b) Recently the use of thiopentone combined with a short- or ultra-short-acting muscle relaxant has become common. To my mind this method is one of the most reliable in experienced hands, provided a big enough dose of each agent is used. It is better in this method to err on the side of overdosage rather than of underdosage, and I find Flaxedil to be better than the ultra-short-acting muscle relaxants since it also partly blocks the vagal nerve supply from the trachea and lessens the tendency to strain on the tube. With a little practice it is usually possible to gauge the dose so that the patient is breathing again three to five minutes after the injection.

If this method of thiopentone plus a muscle relaxant is used it is necessary not to delay in instituting controlled respiration, lest anoxia or CO_2 accumulation should develop. The same method works with infants as they tolerate both drugs well. It is preferable to send them to sleep with a little nitrous oxide first, however, as they are sometimes very frightened of a needle. The dosage of thiopentone and Flaxedil necessary for an infant is usually large in proportion to the body-weight.

It is possible to intubate patients under local analgesia; but it is seldom necessary to resort to this.

Endotracheal tubes: First, kinking of intratracheal tubes is more likely to occur in this than in any other branch of anæsthesia as usually the surgeon requires the head to be in an unnatural position. It is often advisable therefore to use some form of armoured tube with a wire spiral incorporated in the wall. Portex tubes are less likely to kink than rubber ones and are very satisfactory for most types of case, except that the walls are rather thick.

Second, the bore of the tube is of great importance. Should the operation be a long one, the resistance to respiration will become increasingly embarrassing with time, and may lead us into that vicious circle to which we have already referred. For this reason, except in special cases, oral tubes are better than nasal ones as they are less limited in size. A No. 10 Magill oral tube can almost always be used without distending the larynx. Third, the length of the tube is important, as there may be considerable movement of the head and neck during the positioning of the patient, which may pull the tip of the tube out of the larynx or push it down the right bronchus. The chances of this happening are minimized if, with the head in the normal position, the tip of the tube lies half-way between the larynx and the coryna.

The patient having been intubated, it is still necessary to keep the plane of anæsthesia fairly deep until he has been finally settled on the table in the correct position, as the head is bound to be moved about while this is being done. As soon as possible the patient should be tilted with the head up and feet down, to give the maximum time for any venous congestion to disperse from the head.

Once the scalp flaps have been cut and the patient has settled down, the anæsthesia can be gradually reduced and eventually 50 : 50 N_2O : O_2, with occasional small doses of thiopentone and perhaps minimal trichlorethylene, is all that is required to produce a state which is almost physiological sleep. This, however, is insufficient for work on the trigeminal ganglion, or round the base of the brain or the falx, as all these structures are sensitive to painful stimuli.

III. If the Dura has to be Opened When the Intracranial Pressure is High, Damage May Result

There is a danger of strangulation of the cortex, a knuckle of which may be extruded. If the incision is not rapidly enlarged, it may be strangulated. This is very serious in an evocative area. Should the pressure below the tentorium be high and should there be a block in the aqueduct of Sylvius, an upward coning may occur when the supratentorial pressure is suddenly lowered by an opening of the dura or by a tapping of the ventricles. The converse of this, downward coning, may also occur if the posterior fossa is opened when the supratentorial pressure is high, particularly if the patient is in the sitting position. In any case it is very desirable to lower the intracranial pressure before opening the dura, as it makes everything very much safer and easier. This may be done by tapping the ventricles, but if for some reason this is not desirable or possible, other means must be sought.

In a known high pressure case the hexamethonium compounds can be used with advantage from the commencement of the operation. The effect of these hexamethonium drugs on the C.S.F. pressure depends to a large extent upon the lesion. Obviously, where there is a large solid tumour near the surface, with no block, the effect will be less than when the disturbance is associated with increased vascularity or internal hydrocephalus. By lowering the arterial pressure considerably, the rate of C.S.F. secretion by the choroid is greatly reduced, and there is also a shrinkage of the vascular network of the brain. The advantages of this method are very marked in the removal of hæmangiomata or other very vascular tumours, since the blood loss is cut down to a fraction of what it might otherwise be. Many previously inoperable tumours are now removable under such a technique.

Posture plays a vital part in this matter, for the C6 compounds relax the musculature of the veins as well as that of the arterioles; venous congestion may actually be made worse if the drug is given when the patient is not in a position which allows gravity to drain the blood out of the veins at the site of the operation. C6 may actually increase the bleeding in operations on the spinal cord when the patient is face down on the table, as pressure from the abdomen is transmitted via the lumbar veins to the peridural venous plexus which then becomes very engorged. If C6 is to be used in this position the patient must be supported above the table on sausage-shaped cushions. With the proper use of the hexamethonium compounds, many cases of intracranial hypertension may be so improved that the C.S.F. pressure is atmospheric by the time the dura is to be opened. As the pressure is lowered the administration becomes easier because the respiratory disturbances due to the condition are also lessened. A method personally found to be most satisfactory is the use of a fairly large dose of C6, about 100 mg., at the beginning of the operation. This aims at complete ganglionic block. The fall in blood pressure is then regulated by adjustment of posture. It should be possible to do this without disturbing the surgeon; most of the adjustments can be carried out before the operation begins. The level of blood pressure to be aimed at can only be decided from experience and from assessment of the individual merits of the case. Since this technique was tried, as it has been recently, for operations on most cases of cerebral tumour and in almost all conditions where the intracranial tension was likely to be raised, the operative approach has been very much easier. An index of this is that we find that it is necessary to transfuse only about one craniotomy in twelve instead of each and every one, whilst the operating time has been much reduced, with consequent advantage to both surgeon and patient. We have used it in just over 50 craniotomies; after-effects on the kidneys, the brain and the

cardiovascular system have been carefully looked for but no such complication has arisen for which we can lay the blame on C6. It is avoided in cases of subdural hæmatoma of obvious cardiovascular disease, but it can be used with care in patients up to the age of 65. We have also used it twice for patients operated on in the sitting position, but here great care must be taken to guard against air embolism and against sudden vasomotor collapse. It was only used in these two cases because the intracranial tension was so high as to make it dangerous to open the dura unless the pressure was reduced.

So far, it seems quite clear that the advantages to the patient of using hypotensive drugs in neuro-surgical anæsthesia more than outweigh the disadvantages; they should never be used lightly, or as a routine, but kept for those cases where their use is definitely indicated. One must be constantly on the look-out for signs of cerebral ischæmia or anoxia, such as changes in the blood pressure or the respiration, particularly in the depth of respiration.

It is debatable what one should do about the blood pressure before the dura is closed. We find the most generally satisfactory method is to restore the blood pressure to a fairly reasonable figure, probably subnormal, but adequate to maintain an effective cerebral circulation. This pressure should not be allowed to be exceeded for several hours after the end of the operation as intracranial hæmo-rrhage might occur at the operation site.

At all times during the use of hexamethonium compounds strict attention must be paid to adequate oxygenation. 50%, or even more, of oxygen may be necessary in the inspired gases, for if the circulation rate is reduced steps must be taken to ensure that the oxygen transport of the blood is sufficient for tissue needs. Even the small amount of oxygen which is dissolved in the plasma may be important in this respect.

IV. *Danger of Trauma to the Vital Centres*

The danger signals mentioned by Dr. Hunter indicate that some vital centre is being affected. This may happen in any operation near the mid-brain or the medulla or even in one remote from these regions should the lesion be a large tumour. The signs to be watched for are changes in systolic and diastolic blood pressure and in the pulse and respiration rates. It is obvious, therefore, that when such critical manipulations are going on, it is essential to do nothing with the anæsthetic which may by itself produce like changes, or it will be impossible to tell whether such changes are due to the anæsthesia or to surgical manipulation. Should they be due to the latter cause, instant warning must be given to the operator. It is essential, therefore, to keep the anæsthetic on an even keel using an agent that can be given continuously, rather than one requiring intermittent administration.

V. *The Possibility of Intracranial Hæmorrhage*

By the time the operation has reached this stage N_2O, O_2 and trilene, which can be given at a constant rate for one or two hours, will usually be quite satisfactory. If it is not sufficient a trace of chloroform is a great help. None of these agents by itself is likely to produce any sudden change; whereas intravenous thiopentone or pethidine may do so. It is better to discontinue any trilene or chloroform about an hour before the end of the operation, in order to have the patient as light as possible at the finish. The patient is then watched carefully for several hours, the serial readings of blood pressure, pulse and respiration rate being continued for a day. Whilst there is a steady emergence from anæsthesia, there is little cause for alarm, but should progress be checked or consciousness start to recede, it may be necessary to reopen the flap, re-explore the operation site, and excavate any clot which may have formed.

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Section of the History of Medicine

[*April* 2, 1952]

William Cole (1635 – 1716) and his Discovery of the Spiral Nature of the Intestinal Musculature

By JESSIE DOBSON, B.A., M.Sc., A.C.I.S.

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IT is not often that a fundamental discovery in the scientific world is overlooked for a considerable length of time and still less often does it happen that the same "discovery" should be made anew after a lapse of two hundred and fifty years. This, however, actually occurred in the case of the discovery of the spiral arrangement of the musculature of the intestines. It was on May 22, 1676, that the following paper was published in the *Philosophical Transactions of the Royal Society*: "A discourse concerning the Spiral, instead of the supposed Annular, structure of the Fibres of the Intestins; discover'd and shewn by the Learn'd and Inquisitive Dr. William Cole to the R. Society." In this paper, the "Learn'd and Inquisitive Dr. William Cole" made the following observations. "I conceived it might be doubted . . . how the actuating matter, or impression . . . should be transmitted from one to another down along the whole tract of the Intestines; since Natures usual way, for the propagation of Animal motion, is by a Continuation of vessels . . . from the part where it begins to that to which 'tis imparted, either for the conveyance of some actuating substance, or the communicating an impression." Dr. Cole found it difficult to explain the cause of the propelling force through the intestines if the musculature were annular as had been previously postulated. On the other hand, if a spiral arrangement of the musculature were in fact present, this problem presented no difficulties, for he says: " . . . all muscles are observed to have two tendons, one at each extremity, by the approach of one whereof toward the other, its motion, which is contraction is performed." If, therefore, "those muscles which have been esteemed annular, might perhaps be spiral, and so continued down in one tract to the lowest extremity of the intestines . . . when . . . a bare motion shall be impressed on them at their beginning, or any substance impelled into them, they being to be supposed in statu naturali, moderately tense, so long as the moving cause continues, the motion must be successively continued all along their tracts, and, that being in ambitum, must therefore, whilst it lasts, by abbreviating these fibres, straiten the intestine, and so thrust forward what is contained in it, especially if they proved to have a muscular fabrick". The idea appealed to him the more "for the seeming easiness of the performance; Nature's operations being the most easy and simple that can be imagined, though for that reason very often, I doubt, overlook'd".

Dr. Cole did not, at first, take any steps to find any anatomical proof of this conjecture, but eventually, considering that "t'was too unphilosophical to acquiesce in bare speculation, when autopsy might be consulted", he decided to make some experiments. He first investigated the intestines of an Ox, "which, by reason of their largeness of proportion to those of most other species of animals, seem'd fittest for the tryal; afterwards in those of Sheep and Calves, beside the repetition of it in Oxen, and not only in the smaller intestines, but in the colon and cæcum also". His method of procedure was as follows: "I was fain to cause the intestine of Oxen to be boiled 5 or 6 hours, of Sheep 4; whereby the compages of the parts was so loosned, that the two outward coats, viz. the common one, and that consisting of right fibres were easily separated (if it were attempted soon after it was taken out of the water) from that to which my search was destined, and left those reputed annular ones naked (though, by the way, too long coction would prove prejudicial on the other hand, by too much intenerating the fibres)." Though Dr. Cole found it difficult to separate single fibres, he was able to isolate bundles and, furthermore, though he found it impossible to separate these clusters towards the right-hand side, on attempting to do so towards the left, he could easily unravel them to a considerable length. He further observed during the course of these experiments that in addition to the main bundles there were numbers of smaller fibres which, running obliquely, served to unite the successive spirals together. On investigating the cæcum, Dr. Cole found that it was much more difficult to unravel the fibres "which seemed more interwoven than those of the rest, and to have contrary tendencies one among another". In conclusion, he says: "This is the sum of my observations hitherto concerning this coat, which I take leave to think one concave and Helical muscle (if I may so style it:) And that it might be supposed such, the forementioned insertions seem'd to evidence, they appearing to me in the separating appositely enough to represent the fabrick of a muscle delivered by the accurate Steno. Where the tendons of it are fixed, is not evident; but, if I may have the liberty to conjecture, I should think the upper of them to be radicated (at least) at the pylorus (if not as high as the sphincter gulæ, if this be not it) since, the carneous coat of the stomach being by the learned Dr. Willis found to be a muscular contexture, and there being a continuation of motion between that part and the intestines, it seems to me not altogether improbable they may be but one muscle; and the other at the anus. Whether the supposed annular fibres of the veins and arteries may not have the same fabrick as those of the Intestines, since both these kinds of vessels seem to have a peristaltick contraction of their own, and not to be bare conduit-pipes to transmit the impelled bloud, I propose to be considered and examined by persons of more acute hands and judgment; as I do all what I have here delivered, not daring too much to trust even the informations of my own hands and eyes, till I find them confirmed by those of others, more judicious as well as dextrous in making experiments."

This new observation was entirely overlooked and the stereotyped description of longitudinal and circular layers of intestinal musculature persists in textbooks until this day. It was not until 1921 that Dr. Eben J. Carey's paper in the *Anatomical Record* (**21,** 189), entitled "Studies on the structure and function of the small intestine", completely confirmed the correctness of Cole's observations which, however, were quite unknown to Carey. Carey states that "Because of the left-handed, helicoidal arrangement of the musculature of the intestine, the intestinal movements are comparable to the action of a left-handed screw" and he found that "The inner muscular layer is wound as a close spiral. The outer as an open spiral". This description has not even yet been generally recognized in anatomical teaching and, as has been stated, the musculature of the intestinal wall is still commonly regarded as consisting of an outer longitudinal and an inner circular layer. Carey points out that though Franklin P. Mall seemed to know of the spiral arrangement of the fibres of the submucosa of the intestine (on p. 41 of his paper in the *Johns Hopkins Hospital Reports* of 1896, he says: " . . . it is shown that spirals run in both directions and make a turn in every four or five centimeters of intestine"), apparently he did not appreciate the fact that such an arrangement might explain the peristaltic action. It has been suggested that possibly the long delay in rediscovering the true spiral structure of the intestinal wall might be due to the use of the microscope which, instead of revealing the spiral nature of the muscle fibres, would tend to show them as the "discrete muscular rings" mentioned in most of the textbooks. Cole rewrote his paper on this subject in Latin in 1693 and it was published in London as a short pamphlet entitled "Dissertatiuncula de mechanica ratione peristaltici intestinorum motus ex observatione anatomica; qua ostenditur fibras quae pro annularibus habentur, esse revera spirales".

William Cole was born in 1635 and, having studied medicine at St. John Baptist's Hall, better known as Gloucester Hall, Oxford, took his degree of Bachelor of Physic on August 7, 1660. On July 3, 1666, he became Doctor of Physic. He practised his profession in Worcester and was apparently very successful, being consulted by many persons of note in the neighbourhood. In 1692, however, he moved to London and on June 26, 1693, became a candidate for the Fellowship of the Royal College of Physicians. On June 25 of the following year he was elected a Fellow.

On November 17, 1681, Cole wrote to Dr. Sydenham, the distinguished physician, and the following extracts are from this letter, which Sydenham published in full as an introduction to his "Epistle treating of the Small-pox and hysteric Diseases": "Worthy Sir, You will perhaps wonder

what impertinent person it is that breaks in upon your serious studies; but I hope you will pardon me, when I assure you, that 'tis chiefly to express my due acknowledgment, for the singular advantages I have received from your elaborate essays on *acute diseases*. For you have specified all the constitutions of years and of the air, of which you undertook to treat with great accuracy, and in a quite new method, and have intimated such genuine and very obvious indications, for the cure of all the diseases happening therein, and illustrated the work with such excellent remarks, that we may reasonably presume, that so much sagacity and indefatigable application will eternally oblige both the physicians, and those who shall be committed to their care" ... "You ought therefore, learned Sir, to be esteemed the preserver of mankind, and a sure guide to the sick in the extremest danger, conducting them to health, if they will but follow your directions." Dr. Cole goes on to beg Dr. Sydenham to publish his views on the treatment of smallpox and the "hysteric diseases" and ends his letter: "At least I beg you will not be offended at the request which I, tho' unknown to you, have ventur'd to make, both for my own and the publick good, and that you will reckon amongst the number of those who are most devoted to you, Your obedient servant, William Cole."

This engraved portrait of William Cole is reproduced by kind permission of the
President and Council of the Royal College of Surgeons.

To this, Sydenham replied: "Worthy Sir, Were I to gratify self-love so far as to appropriate to myself the praises which I have no right to, it would be difficult for me not to betray some pride, upon being so highly commended by so great a man, who, tho' an utter stranger to me, is known to the learned world by his excellent writings. But you having honoured me thus far, tho' undeserving it, I ascribe to your civility; for the worthiest men are so formed by nature, that they are not only kind to such as err in trivial matters, but are always ready to give a proof of their candour, by commending those who come far short of perfection. And 'tis with gratitude I own that you have given me a remarkable instance of this by approving my slender endeavours, which were intended to benefit mankind." Sydenham ended the long letter of over eighty pages, dealing with smallpox and the hysteric diseases, with these words: "Pray give my service to our learned friend Mr. Kendrick, who informed me of your affection for me, which I will endeavour to return in the best manner I am able, who am, Worthy Sir, Your most obliged and affectionate Humble servant, Tho. Sydenham."

Cole returned to the country shortly before his death, which took place on June 12, 1716. He was buried at Allesley, near Coventry, and a memorial floor stone still exists in the north aisle of the church there. Dr. Cole is said to have been "learned without ostentation, and polite without affectation". Most of his writings are in Latin and his two main works, "De Secretione Animali Cogitata" and "Novae hypotheseos ad explicanda febrium intermittentium" were widely known on the Continent. His full bibliography is as follows:

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NOTE.—Several books of reference mention that William Cole practised in Bristol, but this statement relates to a different physician of the same name.

Clinical Section

President—HAROLD EDWARDS, C.B.E., M.S.

[*March* 14, 1952]

Green Teeth in Baby Due to Rhesus Incompatibility.—D. J. MACRAE, M.D., F.R.C.S.

History.—Baby born 24.1.50, one week from full term following artificial rupture of the membranes. This was the mother's second child, the first, a full-time normal infant, was born one year previously. During this second pregnancy antibodies were found in the mother's blood at 28 weeks and 32 weeks.

Condition at birth.—Weight 8 lb.; general condition good. No œdema, but spleen and liver both enlarged; jaundice appeared two hours after birth.

Blood findings at birth: Hb 95%. R.B.C. 3,670,000. Coombs test positive; Rhesus positive. Serum markedly jaundiced.

Replacement transfusion performed: 483 ml. blood given—displaced—403 ml. removed.

Blood count after transfusion: Hb 114%. R.B.C. 5,500,000. Coombs test and Rhesus both weakly positive.

Progress.—Second day: Marked icterus; general condition good; no sign of cerebral affection.

Third day: Icterus marked, baby restless and irritable; a little arm rigidity; no neck rigidity. Baby less restless with chloral. Hb 118%; R.B.C. 4,900,000. Coombs test and Rhesus both negative.

Sixth day: Liver and spleen still enlarged, icterus slightly less. No C.N.S. signs; taking feeds well and appears to be progressing normally. Hb 116%; R.B.C. 5,600,000.

Eleventh day: Icterus persistent and of a greenish colour. Liver and spleen slightly enlarged; no irritability.

Fourteenth day: Icterus less but still present. Baby taking well from the breast. Hb 104%; R.B.C. 5,000,000.

At seven weeks: Baby still showing slight icterus. Hb 72%; R.B.C. 3,200,000.

At ten weeks: Baby a good colour; icterus disappeared. Progress good; no C.N.S. signs.

At nine months: Baby looking very well; colour good, liver palpable but spleen not palpable.

At eleven months: The baby's teeth were seen to be erupting with a yellow colour which later became greenish yellow. All teeth were affected in turn.

As the teeth grew it was seen that, near the alveolar margins, they assumed a more or less normal, whitish colour. The effect of this was to give a greenish ring appearance on the teeth.

The child is now of normal development and intelligence but suffers from a nerve deafness, thought to be due to a fall on the head. This has delayed speech.

It was evident that the neonatal jaundice had produced a pigment deposit in the enamel, but that in its postnatal growth the enamel was normal. This green stain can be distinguished from the extrinsic stain associated with the presence of oral sepsis, where chromogenic bacteria adhere to the remnants of the enamel epithelium and stain the teeth near the gum margin. The latter affects only the labial surface of the teeth, whereas with the intrinsic stain of icterus neonatorum both the lingual and the labial surfaces of the teeth are affected.

The degree of icterus which this child developed at birth was not greater than that seen in many others in whom the teeth were not stained. The fact that the jaundice persisted for almost three months may, however, have played a part in causing the staining of the prenatal enamel.

The mother was subsequently delivered of a third child, who also required a replacement transfusion. This child developed slight jaundice, which cleared up in a few days; the teeth which have appeared are normal.

Thoracic Inlet Syndrome with Recurrence of Symptoms after Operation.—H. H. RENYARD, F.R.C.S. (for HAROLD EDWARDS, C.B.E., M.S.).

A. S., accountant, aged 42.

History.—January 1948: Staphylococcal septicæmia diagnosed on clinical and bacteriological findings and response to penicillin. April to November 1948: Intermittent bilateral gynæcomastia treated with testosterone. January 1950: He began to complain of pain and swelling in the left side of the neck. Pain varied in severity, sometimes radiated to the shoulder and breast, and was related to certain neck movements. Occasional paræsthesiæ in left hand. No weakness or clumsiness. Swelling and cyanosis of hand noticed in certain positions.

On examination.—There was a diffuse swelling below and above the left clavicle, with dilatation of the superficial veins of the chest wall. The left shoulder was held higher than the right and the left trapezius and levator scapulæ were prominent. The left hand was cyanosed, but there was no sensory

or motor loss. There was a pulsatile swelling in the line of the subclavian artery, just below the clavicle and in that position there was a systolic thrill and murmur. On elevating the limbs, the left radial pulse disappeared, and on pressing back the shoulders the left hand became blue.

Investigations.—An infra-red photograph of the chest confirmed the presence of large venous tributaries on the left side (Fig. 1). A retrograde arteriogram, taken at the Royal Free Hospital, showed, in

Fig. 1.—Infra-red photograph of chest, showing dilatation of veins on left side.

a series of seven films, a constriction in the subclavian artery, as it passed over the first rib behind the clavicle. There was no dilatation of the vessel beyond this (Fig. 2). A phlebogram, made by injecting dye into the basilic vein, seemed to show that the subclavian vein was occluded and that a widespread collateral circulation was present (Fig. 3).

Operation, January 1951 (Mr. Harold Edwards): The subclavian vessels were explored by an anterior incision. The artery was normal in calibre and structure and further exposed by dividing the scalenus anterior insertion. Beneath the artery was a tight tendinous band, which seemed to be an additional portion of scalenus medius. It was a definite structure and was situated exactly at the notching of the vessel seen in the arteriogram. It ran between the lower trunk of the brachial plexus and the artery and corresponded to what has been described as scalenus intermedius of Testut or minimus of Albinus. The band was divided and its upper end immediately retracted out of reach. The subclavian artery became more mobile, and no other constricting structure could be demonstrated. The subclavian vein was not obvious.

Progress.—The patient was symptom-free for ten months, but since October 1951 the pain has recurred, the chest wall is again swollen and the hand has become blue. Examination shows that the pre-operative signs have recurred.

Comment.—This patient shows some of the vascular phenomena associated with a thoracic inlet syndrome. It was pointed out by Eden (1939) that although aneurysmal dilatation of the subclavian artery was often diagnosed clinically, it was rare to find such a swelling at operation. Besides claiming that the distal vascular symptoms were due to direct arterial injury, internal thrombosis and embolism, he stressed that division of bands was insufficient to cure the condition. Falconer and Weddell (1943) described three cases in which the artery was damaged by pressure of the clavicle on the first rib in certain positions of the arm. They described a case with unilateral symptoms, with bowing of the clavicle on the sound side, due to old fracture. In their cases the scalenus anterior was paralysed with novocain and yet the symptoms persisted. Telford and Stopford (1931) showed that, in a small percentage of people, the sympathetic fibres in the upper limb may be collected in a separate bundle of non-medullated fibres in the lower trunk of the brachial plexus. They thought that pressure irritation of these fibres caused peripheral vasoconstriction.

The present case was shown to point out the vasoconstriction over the first rib shown by arteriogram. Now that the symptoms had returned, ideas were welcomed as to further treatment. If removal of the first rib was advised, which method of approach should be used?

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FIG. 2.—Retrograde arteriogram, showing notch in subclavian artery as it passes over first rib.

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FIG. 3.—Phlebogram showing non-filling of main subclavian vein and collaterals filled.

(H. H. RENYARD: *Thoracic Inlet Syndrome with Recurrence of Symptoms after Operation.*)

Discussion.—It was pointed out that this might be a case of subclavian thrombosis following staphylococcal septicæmia and the present symptoms and physical signs were in keeping with this. The constriction of the subclavian artery shown in the X-ray might be a normal appearance. It was thought that the patient's present symptoms did not warrant further operation but that he should be kept under observation.

Malignant Hypertension in a Girl of 16.—ROBERT H. CUTFORTH, M.D., M.R.C.P. (for FRANCES V. GARDNER, M.D.).

The patient first attended hospital on 2.2.52 complaining of headaches for two years. For three months they had been severe and occurred every morning, and for three weeks she had noticed loss of appetite and complained of frequent morning vomiting. The headaches were frontal and temporal. They were present on waking in the morning but had usually disappeared by 11 a.m. There were no other complaints at this time.

In her previous history there was no suspicion of renal disease, and her only illness was tonsillectomy complicated by a hæmorrhage at the age of 12 years.

On examination.—She appeared to be a cheerful girl of normal development. Pulse 120 per minute, regular. Blood pressure 290/200 mm.Hg. Left ventricle enlarged clinically and on screening. A triple rhythm was present at the apex.

Optic fundi: Old and recent exudates, fresh hæmorrhages and severe papillœdema present on both sides.

There were no other abnormal physical signs in any system.

Urine: A faint cloud of albumin. No casts. One or two pus cells and red cells per low power field. Blood urea 32 mg.%.

Urea clearance 21·9% of average normal.

Intravenous pyelogram: Poor secretion on both sides, but the outlines appear normal. No evidence of unilateral renal disease.

On these findings, a diagnosis of malignant hypertension was made since it appeared that the obvious alternatives of chronic nephritis and unilateral renal disease had been excluded.

Early during her stay in hospital, the patient developed lower mid-line abdominal pain with tenderness in both iliac fossæ, this being associated with constipation.

The main problem in this case was the question of treatment. A sodium amytal sedation test done shortly after admission gave such unsatisfactory results that it was thought wise to avoid sympathectomy until other methods had been proved a failure, and so hexamethonium bromide was tried nine days after admission.

The effect of the intravenous test dose of this drug was encouraging, and can be seen in Fig. 1, 38 mg. lowering the blood pressure to 165/125 mm.Hg. The patient was therefore given 40 mg. subcutaneously six-hourly. Twelve hours later she developed blurred vision, abdominal distension and
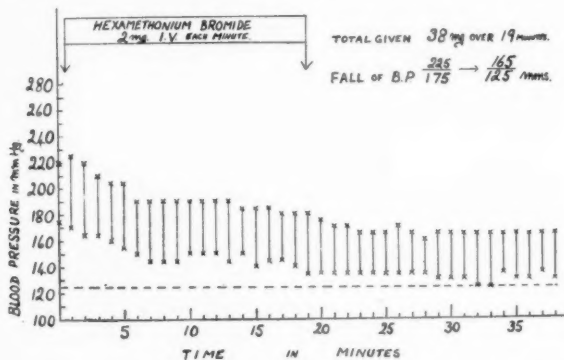
FIG. 1.—Intravenous hexamethonium bromide test.

a few minute petechiæ appeared on the skin, and after a further twelve hours and a total dose of 120 mg. she vomited blood and passed a melæna stool. The hexamethonium bromide was discontinued and, after a short interval, Veriloid was used in its place. It is of interest to record that her abdominal pain completely disappeared after she had passed the blood and has never recurred. Complete blood examination and a barium meal and follow-through have failed to reveal any cause for the bleeding, though her stools are still positive to the benzidine test.

Veriloid given as an intravenous test proved even more effective than the hexamethonium compound, the blood pressure falling to 165/115 (*see* Fig. 2). The drug was given by mouth, starting at 9 mg. daily in divided doses and increasing by 1 mg. each day.

This régime has been successful in the following particulars. First, it has stopped the vomiting and also the headaches to the extent that the patient is now symptom free. Secondly, it has brought about an improvement in the optic fundi. The soft exudates and many of the hæmorrhages have been

absorbed and the papilloedema has decreased to quite a marked degree. Thirdly, it has, to some extent, controlled the blood pressure without unpleasant side-effects but, as can be seen in Fig. 3, this control is becoming less effective in spite of the increasing dose of the drug.
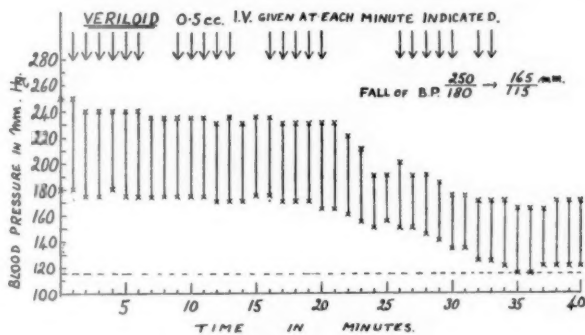
Fig. 2.—Intravenous Veriloid test.
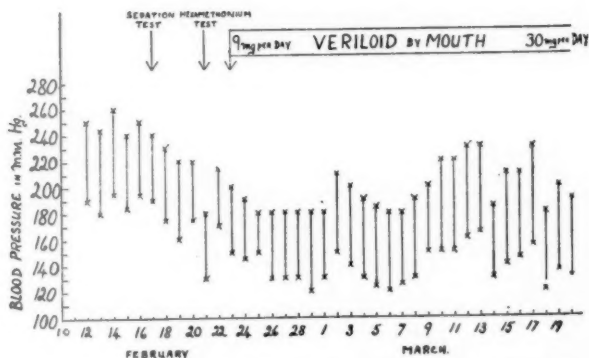
Fig. 3.—Daily blood pressure chart throughout patient's stay in hospital.

The remote possibility that this girl might have a phæochromocytoma and not be a case of malignant hypertension at all has been investigated. The intravenous histamine test and the piperoxane test are both negative, and by the courtesy of Dr. W. S. Peart, the patient's urine has been estimated for the presence of pressor substances. These have been found only in normal amounts, indicating that a phæochromocytoma is unlikely to be present.

Dr. K. Gurling: I would have thought the melæna from which Dr. Cutforth's patient suffered was certainly due to the malignant hypertension. Arteriolo-necrosis certainly occurs in the vessels of the small bowel and I saw a fatal hæmatemesis in a patient with malignant hypertension only two weeks ago. He was a young man, aged 26, who had undergone sympathectomy for well-established malignant hypertension with papilloedema, two years previously. The blood pressure rose and a month before his death it was 280/150. He had a severe hæmatemesis ten hours before his death and again shortly before admission to hospital when he was exsanguinated. He died before any treatment could be given.

Post mortem there was widespread arteriolo-necrosis and the vessels of the ileum were particularly affected. There were many petechiæ in the stomach and small bowel but no actual ulcer from which a hæmorrhage might have occurred. Another interesting feature was the presence of a small pontine hæmorrhage. The arteriolar damage appeared to have occurred in several places at once and was of a severe degree. There was no question of the blood coming from his nose or lungs and the gut contained a considerable quantity of altered blood.

The melæna, therefore, seems to be an unfavourable prognostic sign.

Case for Diagnosis: ? Chronic Pancreatitis.—ROBERT H. CUTFORTH, M.D., M.R.C.P. (for Sir DANIEL T. DAVIES, K.C.V.O., M.D.).

Woman, aged 71. Sent to Out-Patient Department, Royal Free Hospital, as an old-established case of pernicious anæmia with subacute combined degeneration of the cord, complaining of abdominal pain and vomiting for five months.

Past history.—1935: Admitted to another hospital severely anæmic. Hb 18%. R.B.C. 800,000. C.I. 1·2. W.B.C. 2,400. Diagnosed as pernicious anæmia and treated with liver injections. No record of test-meal.

September 1939: Noticed loss of appetite. November 1939: Noticed her skin becoming yellow. In December admitted elsewhere deeply jaundiced with pale stools and dark urine. No history of pain at this time.

December 1939: Laparotomy.—Dilated gall-bladder present, but no obstruction found. Hard liver, no stones, no evidence of growth in liver, gall-bladder or stomach, pancreas appeared normal. A liver biopsy was taken and a cholecystoduodenostomy was performed.

Report of liver biopsy: "Multiple nodular hyperplasia."

The jaundice cleared, and she continued with her liver injections, remaining well until October 1951 when she began to vomit occasionally and to have intermittent epigastric pain.

November 1951: The report of a barium meal states: "Delay in emptying of stomach, one-third residue present at five hours." "A primary diverticulum of the convex border of the duodenum" is described.

February 1952: Sent up to Royal Free Hospital because of the increasing severity of the vomiting.

On examination.—A thin, frail old lady who had lost 1 st. 4 lb. (8·2 kg.) in six months; she was grossly dehydrated and had a tender mass in the upper abdomen, to the right of the mid-line. This mass did not move on respiration, was associated with guarding, was about 3 in. (7·5 cm.) long, 2 in. (5 cm.) broad, and seemed to be in the region of the duodenum. The stomach was distended and splash could be elicited. She had the signs of peripheral neuritis in the legs, but the plantar responses were equivocal. She was admitted and treated with intravenous drip saline, and gastric aspiration for a few days, and a barium meal was then done. This showed an old cholecystoduodenostomy with narrowing of the duodenum in this region. The primary diverticulum reported in the previous X-ray was seen to be the gall-bladder. The *blood count* at this time was: Hb 57%, R.B.C. 2,990,000. C.I. 0·97. W.B.C. 4,300. Stools: Occult blood test positive. Test meal: Free acid and pepsin present.
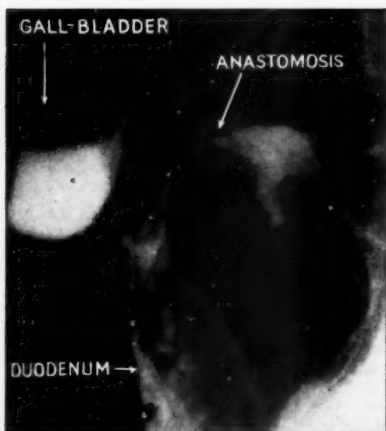
FIG. 1.—Barium meal showing the duodenum and the gall-bladder with fluid level.

The obvious diagnosis to make in a woman of 71 who presents with a story of vomiting and loss of weight, and who has an upper abdominal mass, is, of course, a neoplasm. In this case, however, there seemed to be several reasons why a more cheerful view of the situation was justified. First was the degree of improvement which took place after a few days in hospital: the vomiting diminished, the guarding disappeared, the tenderness was less. The barium meal, taken shortly after admission, did not give the appearances one normally associates with a neoplasm, and suggested an inflammatory mass in the region of the old cholecystoduodenostomy (*see* Fig. 1). Added to this was the past history, the attack of jaundice of unknown cause in 1939, the anæmia and neuropathy, which, in view of the test meal findings, certainly did not seem to be pernicious anæmia and subacute combined degeneration of the cord.

It was tempting, therefore, to connect up these incidents and to postulate that the anæmia was intestinal or hepatic in origin and associated with a pancreatitis with occult steatorrhœa and that the attack of jaundice and the palpable mass were all differing manifestations of the same condition.

At laparotomy, however, this mass was found to be a very fibrotic anaplastic carcinoma in the region of the pylorus in close association with the old anastomosis to the gall-bladder. The liver and pancreas were normal. It is probable that the original jaundice was due to infective hepatitis and that the anæmia and neuropathy, which have both been present for sixteen years, represent a separate diagnostic entity.

Section of Psychiatry

President—DESMOND CURRAN, F.R.C.P., D.P.M.

[*February* 12, 1952]

DISCUSSION ON THE ROLE OF THE PSYCHOLOGIST
IN PSYCHIATRIC PRACTICE

Dr. Henry Wilson: The psychiatrist can afford the psychologist material for his researches, and should welcome the psychologist's clinical researches as well as his general ones. But he would be well advised to hesitate before asking the psychologist to do his work for him. He would be wise to exchange ideas with the psychologist every so often, but too intimate connexion may be wasteful.

In diagnosis: No psychological tests exonerate the psychiatrist from not using all his clinical acumen, and his powers of observation as to what the patient does, as well as what he says. The psychiatrist may well use the Matrices, or Kent's Oral Scale to discover how the patient sets about his task, as well as relying upon the psychologist to present him with some arithmetical result. The psychologist reveals much in his reports upon children which the psychiatrist is not trained to elucidate. But we must be critical when the academic psychologist presents clinical theories. With adults the most useful question is "Has this patient the capacity for this job, or not: or are his powers superior to his work?"

The criteria which are so usual and often so helpful "What standard did this person reach at school?" "Has he been regular at his work, or not?" must be augmented by a consideration of social dexterity. Some defectives are able to hold managerial jobs because of their capacity in a limited situation. This subject is in need of urgent clarification.

The weight of the protocols attached to any case may be in inverse proportion to their usefulness. The clinician's job is to keep his tools for diagnostic acumen sharpened and to ask of every request for outside help "Is this really necessary?"

Tests which either psychologists or psychiatrists could devise, and which would enable us to say "These symptoms are intensified because there is a search for gain" would be welcome, though even then we might find that our gains were illusory. They might lead us to seek a dualism between mind and matter which would still elude us. The same difficulty presents itself over the problem of "neurotic overlay". We long for clarity; every new step in knowledge seems to emphasize the intricacy of Body-Mind relationship.

The more complicated test procedures like the Rorschach test become dangerous if we see in them firm answers to our questions. They can be useful in getting the patients to talk about certain problems which will enable us to discuss urgent concerns more easily. On the other hand, the simple test procedures, which have been so well illustrated by Professor O. L. Zangwill's work, should be at the service not only of our neurological colleagues, but of ourselves. The openings of such test procedures to increasing numbers of organic cases which a psychologist is liable to see, should be considered; the necessity of understanding more about the characteristics of electrical convulsant therapy transient traumatic memory loss should be stressed. The same is probably true of the help the psychologist can give us over types of psychopathic personality, and mathematical procedures used by the psychologist might make assessment of prefrontal leucotomy results clearer, if we could straighten our basic thinking.

Mr. O. L. Zangwill (*Institute of Experimental Psychology, Oxford*): As I see it, the problem under discussion has two main roots. On the one hand, there has been a steady demand on the part of psychiatrists for more standard and objective methods of assessment, particularly in the intellectual sphere. This demand, which goes back at least to the time of Binet, shows no signs of receding and it is incumbent on psychologists to attempt to meet it. On the other hand, there has been some impatience on the part of psychologists with the somewhat narrow attitude to mental life fostered by the academic laboratory, coupled with a wish to seek more fertile and socially relevant fields of inquiry. In view of these two complementary demands, it is not surprising that many psychologists have responded eagerly to the opportunities which have become available to them in recent years for work in the clinical sphere. In what follows the term *clinical psychologist* applies to suitably qualified non-medical workers in the clinical sphere of psychiatry.

I think it is fair to state that, in this country at least, co-operative work between psychiatrists and clinical psychologists has made a good beginning. Although originally limited to Child Guidance and kindred services, it has developed widely in recent years and an appreciable number of psychologists are now actively employed in mental hospitals, neurosis centres and neuropsychiatric departments. Although their co-operative work with psychiatrists shows many signs of healthy growth, it would be idle to deny that problems and difficulties exist.

Some of these difficulties, most notably the reliance of certain psychologists upon somewhat abstruse mathematical procedures, have already been touched upon by Dr. Henry Wilson. Although mathematical methods are inseparable from certain aspects of experimental psychology, they should, I think, be properly regarded as tools in research rather than ends in themselves. When used with due consideration of the nature of the problem under inquiry they may, as Crown (1952) has pointed out, lead to real advances in the elucidation of problems of genuine clinical import. There are, however, many further difficulties which demand our consideration. A number of these, fortunately, I believe to be more apparent than real. They have their roots in certain misapprehensions on both sides which have not always been made explicit. The purpose of my communication is to state what I believe these misapprehensions to be, and to indicate how we might perhaps free ourselves from them.

In the first place, I believe that misapprehensions of some gravity attach to the nature and function of *psychometric testing*. The psychiatrist, perhaps unduly impressed by the prestige of numbers, is apt to see in test scores valid, quantitative measurements having the same kind of finality as biochemical or serological test findings. In view of the uncertain rationale of psychometric methods, such confidence is, in my opinion, ill-placed and misleading. Whereas the actual test procedures are designed in accordance with scientific method, and are to that extent acceptable to all psychologists, profound differences of opinion exist as to the meanings which should most properly be attached to test findings. There is also divergence of view as to the most expedient methods of administering mental tests in the clinical setting (Shapiro, 1951). At the present time, therefore, psychometric testing is better described as a technology than as an applied scientific discipline (Zangwill, 1950). It has great value in psychiatry but may become a real danger if accepted in too uncritical a manner. If, however, the psychologist is content to regard his tests as pragmatic, tentative tools, and the psychiatrist to accept his findings with appropriate reserve, no serious misunderstandings on this score need arise.

Secondly, the *nomenclature* employed in psychological writings has given rise to a good deal of misunderstanding. This can be well illustrated by reference to the concept of *deterioration*. In his clinical estimates of deterioration, the psychiatrist lays stress on practical and social judgment, clarity and relevance of thought, and general efficiency of memory. The psychologist, on the other hand, tends to use this term in a more restricted sense, often attempting to define it operationally in terms of differential test performance (Wechsler, 1944). As a result, a psychiatrist may say that a given patient is not deteriorated and a psychologist aver that he is. Not uncommonly, both are right but mean different things. In my own experience, such discrepancies are particularly liable to arise in the assessment of post-traumatic intellectual loss. We have seen many patients who, on psychometric evidence, have clearly fallen below their previous levels of ability in certain fields of performance. This is undeniably deterioration. At the same time, the degree of impairment may be too slight or specialized to make itself apparent on clinical examination and prove to be of little or no significance in daily life. From the clinical point of view, therefore, there is no deterioration. It is plain that such differences of opinion, unless carefully scrutinized, do not tend towards mutual confidence and it is important that we should recognize their origin.

Semantic difficulties have also been responsible for certain criticisms which have been levelled against the work of the Maudsley group of psychological workers. Terms such as suggestibility, introversion and neuroticism have been defined by Eysenck (1947) in an extremely precise way in keeping with his extremely precise methods of investigation. But these very same terms have a much wider connotation in clinical psychiatry, opening grave possibilities for mutual misunderstanding. It must be recognized that many psychological terms at present do double duty and that a common vocabulary can only grow out of close common endeavour.

Thirdly, differences between the psychiatric and psychological viewpoints are extremely prevalent in the sphere of *research*. This, to my mind, places most unfortunate obstacles in the way of an integrated attack on basic problems in psychological medicine. In the past, much research work done by psychologists and psychiatrists within a given field of inquiry has been additive rather than integrative. A good example of such additive research is furnished by the massive Columbia-Greystones inquiry into the effects of selective partial ablations of the frontal lobes (Mettler, 1949). In this study, there was certainly a pooling of research techniques, but no attempt to devise fresh methods of investigation on the basis of a genuine inter-disciplinary attack. In my opinion, an integrative approach to research involves more than mere juxtaposition of techniques and skills. It demands genuine interaction between people of different training and background leading to original advance in descriptive or theoretical study.

In the sphere of neurology, prototypes of the integrative kind of research which I have in mind already exist. One may recall the long series of published studies by Gelb and Goldstein (1920) on the psychological after-effects of brain-injuries. In this country, the co-operation between Sir Henry Head and Sir Frederic Bartlett on the aphasia problem, while it led to no joint publication, profoundly affected the views of both men in their respective fields of inquiry. More recently, we have witnessed useful co-operation between neurologists and psychologists on a variety of special problems, e.g. the work of Smith and Akelaïtis (1942) on section of the corpus callosum and that of Bender and Teuber (1947, 1948) on disorganization in the visual sphere. These studies are collaborative in the best sense of the term and give hope of narrowing the gap between experimental and clinical methods in neuropsychiatric research.

It is unfortunate that integral co-operation of this kind appears rarer in psychiatry than in neurology. This appears to be due, in part at least, to basic differences between medical and non-medical psychologists in their approach to psychological issues. The present-day psychologist draws his impetus from the experimental methods which began with Wundt, and places stress on the comparative, genetic and statistical approaches to his subject. As is well known, these methods have met with most success in regard to the intellectual aspects of personality; their applications to problems of emotion and character have so far remained slender. The psychiatrist, on the other hand, has found the non-experimental, descriptive and systematic types of psychology, associated particularly with the names of Bleuler, Freud, Janet, Adolf Meyer, Jung and Kretschmer, more nearly to his taste. These psychologies deal principally with the broader, dynamic aspects of behaviour and, although erected on an empirical foundation, can seldom be submitted to direct experimental scrutiny. It would seem evident that, if integrative research is to develop, a framework of psychological knowledge common to both psychiatrists and psychologists should be created as its essential condition. Professor Aubrey Lewis (1950) has written that: " . . . in the main, the future (of psychiatry) must be determined by progress in our knowledge of physiology and biochemistry, sociology, genetics and, most of all, psychology." In so far as the last-named discipline is concerned, I submit that such progress will not come about if we have *two* psychologies—psychiatrist's psychology and psychologist's psychology —having little in common apart from the name. Full co-operation in research must develop from common thinking and discussion and this, in its turn, demands a background of appropriate instruction in the Universities and Medical Schools. Such instruction must affect both psychiatrists and psychologists and should set the stage for mutual comprehension in methodology, clinical practice and research. Only in some such way as this can we hope to overcome the difficulties which at present keep us apart and come to understand one another's point of view.

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Dr. H. J. Eysenck: *The psychologist as technician.*—This implies first and foremost an acceptance on the part of the psychologist of the *conceptual system* of the psychiatrist (Eysenck, 1950*a*); it implies secondly an acceptance on the part of the psychologist of the *practical methods* used by the psychiatrist. Before the psychologist can do either, it will behoove him to consider the evidence.

If we consider first the conceptual systems used by psychiatrists, I think psychiatrists would be the first to agree that they are makeshift, *ad hoc*, and frequently inconsistent. To take but one example, it is clearly of the utmost importance to come to a decision regarding the problem of the relation between mental normality, psychosis, and neurosis. There are three plausible alternatives—these three states may be qualitatively different, as tuberculosis is different from hæmophilia; they may differ quantitatively in one dimension, as in the Freudian system, where psycho-sexual regression defines a unidimensional continuum from normal through neurotic to psychotic; or they may differ quantitatively in several dimensions, say one dimension of neuroticism, the other of what we may perhaps by analogy call psychotocism. These are three quite distinct theoretical models; they cannot all be correct, although they may of course all be false. Yet most textbooks of psychiatry adopt all three models simultaneously, using concepts implying qualitative differences in one connexion, concepts implying quantitative differences in another (Eysenck, 1952*b*).

What is true of this problem is true of many other fundamental problems in psychiatry. They are usually dealt with implicitly, rather than explicitly; they are shelved, rather than solved.

This brings us to the second point implied in the psychologist's acceptance of the technician's role—his acceptance of the practical methods of the psychiatrist. And here again, let us very briefly look at one particular problem, that of psychotherapy, from the point of view of the available evidence.

Implicit in the practice of psychotherapy, whether Freudian, Jungian, or eclectic, is the belief that treatment will lead to an amelioration in the neurotic disorder which has prompted the patient to seek help. I have made a review of available reports in the literature dealing with the effects of such therapy (Eysenck, 1952*a*). Excluding psychotic and various doubtful cases, it appears that approximately two-thirds of the patients so treated were considered to be either cured or considerably improved by their physicians. However, while the improvement follows the therapy, it may not be caused by it. We must have what psychologists usually call a control group, i.e. a group as far as possible identical with the experimental group, but which is not submitted to the experimental treatment under investigation. There are fortunately two excellent studies—by Landis (1938) and by Denker (1946)—in which large numbers of severely neurotic patients were treated, not by any form of orthodox therapy, but either by custodial care or by ordinary G.P. treatment. The percentage of recoveries and considerable improvements was again two-thirds of the total sample. As the criteria of improvement were just as stringent in these two control studies as they were in the experimental studies, it is difficult to interpret these figures as lending any support to the claim that psychotherapy has a favourable effect on psychoneurotic disorders. They do not disprove the possibility of psychotherapeutic effectiveness, but they do quite definitely throw the burden of proof on to those who assert it.

It will be apparent why many psychologists are unwilling to accept the conceptual systems or the working methods of the psychiatrist as delimiting their activity. They feel, as indeed do many psychiatrists, that there is much that is unsatisfactory in both, and that the only reason which would justify their acceptance of both concepts and methods would be the immediate demands of therapy. That this assumption is correct is shown by American experience; there psychologists are in many cases called upon to act as therapists, and almost in every case this has made them accept *nolens volens* the concepts and working methods of the psychiatrist. Even in that part of their work which is not directly concerned with treatment, they have attempted to model themselves on the psychiatrist, and have used tests and techniques borrowed from psychiatric history, such as the Rorschach or the Thematic Apperception Test. And even in their research, this subordinate relationship has become apparent; they have usually taken for granted the precepts of some psychiatric system and nosology, and have adjusted their problems within the limits of such a system (Eysenck, 1950*b*).

This position must inevitably lead to conflict and friction between psychiatrist and psychologist. As therapy is the most prestige-giving part of the set-up in which the psychologist works, he will more and more concentrate on this aspect of his work, thus invading more and more the territory of the psychiatrist. Strong defence reactions will be set up—and have in fact already been set up in the United States. Under these conditions psychology cannot make an independent contribution to psychiatry, having accepted psychiatric concepts and methods in all material aspects; nor can it completely merge with psychiatry, lacking the essential medical background of the latter discipline. A more frustrating position for the clinical psychologist is difficult to imagine.

What of the alternative? As I see it, the psychologist should try to complement, rather than imitate, the psychiatrist. His basic training is in science, not in therapy, and he can make his greatest contribution by constantly stressing this aspect of all the problems which he may encounter. Science implies objectivity, abstract thinking, verification, experiment, and even mathematics and statistics. The attitude and training implied in these terms do not usually go with the will and the ability to be an efficient therapist. The obvious course would appear to be a differentiation of function similar to that which has taken place between the physician and the physiologist. It is this conception of the psychologist as an independent scientist, having his own professional standards, methods, and qualifications, which I would like to put forward, as opposed to the one which relegates him to the status of a technician grinding out Binets or Rorschachs in terms of a system the concepts or methods of which are not for him to question.

Both disciplines have enormously to gain by the closest co-operation. The close contact the psychiatrist has with his patients, and his powers of observation sharpened in long years of constant practice, are invaluable sources of theories and hypotheses for the psychologist; the obstinate scepticism of the psychologist, and his stress on experiment and verification, are invaluable safeguards against the easy acceptance of falsehoods and the "premature crystallization of spurious orthodoxies".

In thus accepting the scientific role, it seems to me inevitable that the psychologist must renounce the therapeutic ambition. Instead of aiming to carry out therapy himself, he should rather interest himself in such problems as the measurement of the effects of therapy, the comparison of different methods of therapy, the prediction of therapeutic success on the basis of psychological tests, or the question of the relative importance of method of treatment and person carrying out the treatment. All these questions, I would submit, are of the greatest importance to psychiatry; all of them, I believe, are capable of a solution along lines already tried and found promising. All of them, in addition, are of great theoretical importance in terms of general psychology; they can throw considerable light on the efficacies of rival theories in the field of learning, as well as in that of personality organization.

But these are broad lines of advance, and the psychiatrist may rightly ask whether the psychologist cannot aid him in the day-by-day execution of his duties. Here again, I would tend to disparage the routine administration of tests, which has become such a feature of American practice, and insist that even in this situation the scientist should proceed according to the dictates of scientific method, by regarding the particular case referred to him as a problem, by formulating hypotheses to account for the odd or inexplicable features of this problem, by systematically making deductions from these hypotheses, and by submitting these to rigid experimental tests. The foremost exponent of this method of approach in this country has been Shapiro (1951).

In both the fundamental as well as in the applied part of his work, I hold it to be particularly essential for the psychologist to relate his work to the well-established principles of general psychology. It is from these that his special competence derives; it is to these that he must look for guidance and explanatory hypotheses. His thinking about any specific psychiatric problem will be clarified when this problem can be brought under experimental control, and into contact with concepts and theories with which he is familiar. The term "anxiety" tends to be vague and free-floating; once it can be shown, as has been done by Taylor (1951) and Spence *et al.* (1951), that there is a close relationship between "anxiety" and the speed with which conditioned reflexes are formed, we immediately approach the possibility of operational definition, experimental control, and explanatory subsumption under a better-understood and more widely applicable concept. Indeed, such an approach may in due course lead to suggestions for novel therapeutic procedures; thus by making deductions from the principles of conditioning and learning theory, Mowrer (1950) has been able to show that 100% effective and lasting cures can be achieved for such difficult problems as that presented by enuresis—a striking demonstration, amply confirmed by later work, which contrasts with the much more usual, and much less successful, procedures of the psychotherapist.

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Professor Alexander Kennedy: It is generally agreed that the legitimate demand for the services of psychologists is likely to increase and that their function of applying scientific method to problems of behaviour and of human relations is applicable in a variety of fields of which psychological medicine is only one. In dealing with abnormal function of both mind and brain there is need for a reconciliation of the informed kindliness and clinical art of the physician with the objective efficiency of the scientist and it is my intention to discuss the future of this relationship in the clinical field. If psychiatry is not to become altogether mechanized and dehumanized it may be that the best means of achieving this combination of attitudes will be a division of labour such as has occurred, for instance, between clinical medicine and biochemistry. The psychiatrist, instead of converting himself into an amateur psychologist, must take the psychologist into his team and respect him for his qualities as a scientific worker. Within such a team in my experience the psychiatrist is daily reminded of the need for accurate assessment and his tendency to judge the normal from his experience of the abnormal is restrained, while the psychologist is in turn reminded of the importance of soma as well as psyche. In clinical work the psychologist will constantly be meeting examples of abnormal behaviour where his ingrained tendency to limit the number of variables will give poor results. Medical training produces a toleration of problems in which multiple factors are involved and a facility in the semi-intuitive and empirical methods which are essential in view of the complexity of the human organism and the limitations of our present knowledge. The difference in approach may be seen in its most exaggerated form in those psychiatrists who believe that statistics are the work of the devil and in the psychologist who appears to resent the fact that the human family is not made up of uniovular twins.

As psychiatrists we are naturally deeply interested in the growth of psychology as a profession and have been able to watch the successive stages of pioneer work, of excessive claims and uncritical demand, of the inrush of eccentrics into a new field and of the final emergence of work of great permanent value. We are also seeing the rapid progress which is being made by a professional body dedicated to the formulation of common aims and standards which has shown a strong sense of responsibility to

our own profession and to the public. In recognizing the work which our guests, the British Psychological Society, are doing, our attention is naturally focused on two aspects of their work which are very relevant to our future relations. The first of these is the setting of *standards of basic training* which will, we hope, prepare them for collaboration with psychiatrists and neurologists. The second aspect is the *code of ethical conduct* which psychologists will see fit to impose upon themselves as a profession and in relation to their contacts with the psychologically sick. With the Society we must also be anxious that the public can be helped to get a true appreciation of the function of psychologists and of their background.

So far as training is concerned, we cannot expect to achieve the ideal co-operation with psychologists unless we are willing to help in their training. Quite apart from formal teaching, we have much to offer in the way of clinical facilities and of allowing access to the neurophysiological laboratories and other ancillary scientific services. At the University of Durham, although the Department of Psychological Medicine is not yet five years old, it has been joined by an independent department of Psychology, and my colleague Professor Frederick Smith and I have accepted from the first the principle that psychologists and medical postgraduates should have certain parts of their training in common. We believe that they should have ample opportunities put in their way, through discussions and participation in research, of becoming acquainted with each other's point of view. The psychologist should learn from medical workers about neuroses and psychoses and his training must include actual contact with patients and opportunities for studying social factors in psychological disorder. He should be taught the functional anatomy of the nervous system, preferably by brain dissection and neurological demonstration. The psychiatrist in training on the other hand should learn in the nursery schools—and schools rather than in the hospitals—the psychological development of the normal child, and should learn with the psychologist about industrial conditions, selection and managership. He should be the guest in the experimental psychology laboratory.

The discussions which develop between the two groups are in my opinion most fruitful. Much of the purpose of the preliminary course in basic sciences and methods for the Durham D.P.M. is to practise the medical graduate in the vocabulary of the philosopher, the geneticist and the psychologist and such basic training helps to meet the urgent need, emphasized by Professor Zangwill, for a common language. Even with this amount of interchange the psychologist will not, of course, subsequently undertake the diagnosis of neuropsychiatric disorder. He will necessarily be an ancillary worker in the clinical field just as the psychiatrist is an ancillary worker in Selection procedure and a great part of Industrial psychology.

In psychiatric practice the psychologist is apt in some quarters to be regarded simply as a tester. Psychologists whose work is to carry out prescribed tests may well be needed in some quantity but it is to be hoped that selection for the profession of psychologist will produce a considerable proportion of graduates who will be able to accept much greater responsibility in medical work. The main fields at present in which the psychologist can help are in the assessment of constitutional differences, in special education, in re-education after damage to the nervous system, in the control of cases undergoing speech therapy and in the assessment of the effects of treatment. Very little has been said so far about his place in psychotherapy. In some quarters it seems to be felt that a qualification to practise medicine gives evidence of special ability in dealing with emotional problems. When it is considered how little time, relatively, is devoted to instruction in this subject in the undergraduate medical course the weakness of this attitude becomes apparent. The reason that the management of psychoneurosis is usually in the hands of medical men lies solely in the attitude of trust on the part of the public towards doctors. If, by maturing as a profession and recognizing a strict ethical code, clinical psychologists could achieve, as has the dental profession, a similar position, there seems to me no reason why they should not undertake psychotherapy, once diagnosis has been established. There are certain formal psychotherapeutic procedures, especially with groups, which under psychiatric control might better be undertaken by psychologists, especially in view of the shortage of psychotherapists. Psychologists who, on the other hand, become over-confident of their therapeutic powers, and lose the humility which is essential to sound clinical work, will not be helping their profession as a whole.

In the present phase of scientific research the contact of different approaches frequently produces new combinations of ideas and the co-operation between these two disciplines has already yielded good results. Professor Zangwill has already mentioned a field of research in which there is a great deal more to be done, namely the reduction to terms of measurement of the clinical examination of the nervous system at different levels of integration. Continued work in this field and the incorporation of the results in the training of psychologists will, I think, go far to increase the existing common ground. As Dr. Wilson has pointed out, psychiatrists have only recently gained acceptance in the medical world and psychologists are now following a comparable path. The final relationship which will emerge must be partly the result of planning and partly due to natural evolution of their profession and of its acceptance by the public. That progress can be made more rapid by insisting, as we now do in psychiatry, on proper selection and training of candidates. It is agreed that greater uniformity is necessary in the training of psychologists and the place of the psychologist in psychiatric practice and research will be the more secure if this training includes more contact with clinical psychiatry than it does at present.

Section of ·Pædiatrics

President—Professor A. G. WATKINS, M.D., F.R.C.P.

[*February* 22, 1952]

Dystonia Musculorum Deformans in Siblings. Treated with Artane (Trihexylphenidyl).—BERYL D. CORNER, M.D.

Family history.—The two children described are the only siblings. Parents healthy; no Russian or Jewish ancestry; family have lived in Bristol for several generations. A paternal uncle has had this disease since the age of 8 years. He is now 44 years and completely bedridden.

Case I.—A. S., female, aged 14 years.

History.—January 1946: Started dragging left leg. In the morning she walked normally but by midday walking deteriorated with twisting and kicking of her left leg. Her hands shook on movement. Diagnosed as hysteria: psychiatric investigation carried out at Bristol Child Guidance Clinic. I.Q. 109. Considered to be immature and given psychiatric treatment for six months. She was subsequently seen by Miss D. Kinloch Beck who diagnosed dystonia musculorum deformans.

May 1947: Steady deterioration: now unable to walk by midday; persistent complaints of shaking and unable to hold cup and saucer.

On examination: Much spontaneous movement of athetoid type; both arms tend to be held in extension with considerable muscular rigidity; some lumbar lordosis; hips flexed and adducted: typical torsion spasm of calf muscles on walking, feet plantar flexed and inverted. No sensory changes. Euphoria marked. Reflexes normal.

Investigations.—C.S.F. normal. Lange curve negative. W.R. negative. Liver function tests no abnormality. Electro-encephalogram: Normal rhythms from the pre-central areas. In the post-central regions a very high potential complex delta and theta discharge was seen which was almost completely inhibited by opening the eyes. Alpha rhythm from the occipital lobes was 8–9 c/s and was responsive to physical stimuli.

23.2.50: Very marked deterioration. Her face was expressionless but contortions occurred on attempted movement of other parts of the body. Speech and mastication were impaired. There was permanent flexion of all large joints of the lower limbs, initiation of movement was very difficult and she could only walk 10 yards. She was unable to dress or feed herself except in the early morning, and could only write with considerable difficulty.

Further investigations.—Serum copper normal. No response to therapeutic test with BAL. EEG little change, but occasional spike and wave patterns were elicited.

Treatment (30.3.50).—Artane mg. 2 b.d. was increased to mg. 4 b.d. during a period of three weeks. Dramatic improvement occurred. She was able to undress herself; walking distances much improved; hands much steadier; less muscular rigidity also noted.

Progress.—Treatment has continued with the same dose of Artane (mg. 4 b.d.) In December 1950 she was able to join in school games and walked a mile.

February 1952: All flexion deformities have disappeared: no detectable muscular rigidity: writing completely normal. Child is living a normal active life, can use a sewing machine and swim a few strokes. She has no symptoms and is extremely cheerful.

This case was shown to the Section of Neurology in 1947 by Diana Kinloch Beck (*Proc. R. Soc. Med.*, **40**, 551).

I showed both children to the Section of Pædiatrics in June 1949, at the meeting held in Bristol.

Case II.—R. S., male, aged 11½ years.

History.—October 1948: He was first seen with history of difficulty in walking, late in the afternoon, and for six months tendency to twist right leg. Examination showed definite gait characteristic of early torsion spasm affecting both legs, but right worse than left.

Investigations.—January 1949: C.S.F. normal. Serum potassium 17·2 mg.%. Urine copper excretion 0·0045 mg./ml. (Normal value.)

February 1950: Progressive deterioration. By midday unable to straighten his back and great difficulty in walking; difficulty in change of posture; typical gait and movements of torsion spasm; head and upper extremities not greatly affected.

May 1950: Treatment with Artane 2 mg., increasing to 7 mg. daily. Further increase in dosage produced blurred vision and vomiting. Improvement in gait was noted after six weeks, and the same dosage has been maintained since.

May 1951: Walking normally. Attending ordinary school. Playing games and swims 50 yards.

February 1952: No abnormal physical signs. Only abnormality is a tendency to stand with left hip slightly flexed when he is very fatigued.

Comment.—These children are typical cases of dystonia musculorum deformans (torsion spasm) who at the start of treatment with Artane were severely handicapped and rapidly deteriorating. Artane (trihexyphenidyl) is a synthetic piperidyl compound with marked antispasmodic action on smooth muscle and an inhibitory action on the parasympathetic nervous system. The relief of spasticity in voluntary muscles is considered to be largely due to the latter action as well as to some direct effect on the motor centres. Other atropine-like actions such as mydriasis, control of sialorrhœa, and cardiovascular effects are considerably less than atropine and even in very large doses the cerebral excitation produced does not lead to convulsions. The drug has been most effectively used for the control of tremor and rigidity in parkinsonism; it was therefore hoped that since the characteristic movements and rigidity of dystonia musculorum deformans are also due to degenerative lesions of the basal ganglia this drug might be effective, and this has proved to be the case. Artane is given orally in capsules, three or four times daily, starting with 1 mg. daily and gradually increasing until the effective level is reached, usually 6 to 10 mg. daily.

Dysplasia Epiphysealis Multiplex.—L. G. Scott, M.D., M.R.C.P.

D. K., male, aged 8 years (Born 23.5.44).

He has had vague pains in his legs for the past two and a half years. His legs have been noticed to be thin and the knees relatively large. There is nothing else of significance in the history and no significant abnormality on examination apart from thin legs, flat feet, broad and coarse hands and fingers, and the fact that he is rather small for his age. In October 1951 his height was 43½ inches (average = 46 inches) and weight 40 lb. (average = 49 lb.). His mentality is good and he has not the appearance of a cretin.

Blood chemistry is normal: Serum alk. phosphatase 18 King-Armstrong units; serum phosphorus 4·3 mg.%; blood cholesterol 152 mg.%. W.R. and Kahn negative.

X-rays show changes of dysplasia epiphysealis multiplex in a mild form. There is irregularity in ossification of the epiphyses. The irregularity is both in density and shape. Subsidiary centres of ossification are present around the main centres, resulting in some peripheral stippling but this is not so complete as seen in the punctate type of epiphyseal dysplasia (*see* next case). There is delay in ossification of the epiphyses and only three carpal centres were present on 29.6.51. The external malleoli at the ankle-joints are abnormally low and this has been described in some of the previous cases.

This case has also been seen by Dr. Philip Evans, Professor Alan Moncrieff, and Sir Thomas Fairbank at The Hospital for Sick Children, Great Ormond Street, and I thank them for their permission to publish it.

Comment.—In 1946 and 1947 Sir Thomas Fairbank published papers on a series of 20 cases of what he believed to be a clinical entity and suggested the title Dysplasia Epiphysealis Multiplex. In 1951 he reviewed 26 such cases. The condition is a rare developmental error characterized by mottling or irregularity in density of several of the developing epiphyses, dwarfism, and stubby digits.

REFERENCE

FAIRBANK, H. A. T. (1951) An Atlas of General Affections of the Skeleton. Edinburgh and London.

Dysplasia Epiphysealis Punctata.—L. G. Scott, M.D., M.R.C.P.

C. S., female, aged 4 months (Born 26.10.51).

At birth the child was noticed to have bilateral flexion deformities of the wrists, and she soon became snuffly. Subsequent radiographs of the skeleton showed the appearances of dysplasia epiphysealis punctata. There is widespread stippling of the epiphyses as if they were ossifying from many separate centres. The shafts of the humeri are short and thick and the ends splayed to a marked degree (*see* Figs. 1A, 1B and 2).

This baby has the typical flexion deformities of other joints besides the wrists. On 13.1.52 measurements showed:

Lt. arm at elbow 30° from full extension	Lt. leg at knee 50° from full extension
Rt. arm at elbow 20° from full extension	Rt. leg at knee 60° from full extension

The child has bilateral cataract which is being treated by needling, by Mr. S. S. Freedman.

She also has relative lengthening of the forearms and legs compared with the arms and thighs. Measurements on 8.2.52 were:

Total Height 20½ in.

Both Arms	Both Legs
Tip of acromion to olecranon 3 in.	Greater trochanter to patella 4 in.
Olecranon to styloid process of ulna 2¾ in.	Patella to lateral malleolus 4½ in.

The other systems appear normal, and no thickening of the skin has been noticed.

I wish to thank Dr. E. Owen Fox for the radiographs of this case.

FIG. 1A.   FIG. 1B.   FIG. 2.

FIG. 1A.—Radiograph of the right lower limb at the age of 7 weeks, showing widespread stippling of the epiphyses.

FIG. 1B.—Radiograph of the left knee at the age of 7 weeks.

FIG. 2.—Radiograph of the right upper limb at the age of 7 weeks, showing the characteristic short and thick humerus with marked splaying of each end, as well as stippling of the epiphyses.

Fanconi Syndrome in Two Siblings.—S. B. DIMSON, M.D., M.R.C.P.

Case I.—J. J., boy, aged 4 years.

Admitted to Sydenham Children's Hospital 9.5.51 in tetany: ten days previously had brief attack of dysentery. Enuresis and thirst always. Stools always normal. Appetite good. Parents and 3 siblings (apart from Case II) normal. No consanguinity.

Past history.—Birth weight 7 lb. Teething 8 months. Bow legs noticed at 1 year. Walked at 1 year 4 months. Had cod-liver oil irregularly.

On examination.—Typical tetany. Pallor, fair hair, bossed skull, genu varum, moderate beading of ribs, enlarged epiphyses, distended abdomen. Stools, B.P. and fundi normal. Undersized; infantile behaviour. Weight 19 lb. Height 30 in.

Urine colourless. S.G. 1002–8. Albumin moderate cloud. Occasional granular casts, few R.B.C., few epithelial cells, pH 7·1–8·2. Ammonia coefficient 3·2.

Tetany promptly relieved by Ca gluconate 5 c.c. 10% intramuscularly.

Blood chemistry: Serum calcium 10·6 mg.% immediately after injection: 4·2 mg.% later. Plasma α amino-nitrogen 5·69 mg.% (normal 4–6 mg.%). Plasma phosphates (inorganic) 3·2–5 mg.%. Plasma protein 6·8 mg.% (alb. 5·6; glob. 1·2). Plasma alk. phosphatase 11·3–12·2 King-Armstrong units. Blood chlorides 450 mg.% (as NaCl). Blood sugar (fasting) 75 mg.%. Blood urea 195 mg.%. Serum alkali reserve 38·5 vols. CO_2%. R.B.C. 2,950,000. Hb 52%. W.B.C. normal. Blood group A, Rh positive.

Urine: Cystine present. Benedict's reagent: faint reduction due to glucose, confirmed by paper chromatography. Sulkowitch test negative. Protein: heavy precipitate. Cyanide nitro-prusside: raised cystine. Chromatogram: Gross amino-aciduria, including cystine: Typical Fanconi pattern. Polarograph: Cystine (mg.) / Creatinine (grammes) = 340 (normal 30–45).

Slit lamp: Cystine crystals seen in corneæ, throughout substantia propria and also in conjunctivæ. Visible as a haze to the naked eye.

X-rays: Gross rickets, flattened heads of femora, no obvious osteoporosis, no calcified deposits in abdomen. IVP: failure to excrete dye, none in bladder even after thirty minutes. Cystogram: Bladder filled normally, dye did not enter ureters. Retrograde pyelogram normal. Cystoscopy: Normal ureters.

Treatment.—Ca gluconate intramuscularly for attack of laryngeal spasm nine days after admission. "Adexolin" ⋔ xx b.d. Ca gluconate 25 grains b.d. Ferri ammon. cit. 6 grains t.d.s.

13.6.51: Transfused and put on 1–1½ tablespoons t.d.s. (50–75 c.c.) of Albright's mixture (citric acid 140 grammes, sod. cit. 98 grammes, water to 1 litre), low sodium and high potassium diet.

21.6.51: 600,000 units vitamin D_2 ("Sterogyl") intramuscularly. Ascorbic acid 1,000 mg. daily for seven weeks.

13.7.51: Tocopherol ("Ephynal") 100 mg. daily.

11.8.51: Discharged on Tocopherol, "Adexolin", iron and Albright's mixture.

19.9.51: Calciferol 100,000 units daily in place of "Adexolin".

Progress.—

　Alkali reserve after four weeks of Albright's mixture 50 c.c. daily : 38·2 vol. CO_2%

　Alkali reserve after two more weeks on 75 c.c. daily : 66·5 vol. CO_2%

　Alkali reserve on 12.9.51 on 75 c.c. daily : 57·6 vol. CO_2%

　Alkali reserve on 14.2.52 : 36·1 vol. CO_2%

Blood urea 26 mg.% (20.9.51); 44 mg.% (14.2.52).

1.8.51: X-ray knees and wrists—some improvement in rickets.

11.8.51: Amount of cystine in corneæ is less.

26.9.51: X-ray knees and wrists—rickets has improved further.

January 1952: Much less cystine in corneæ than previously.

13.2.52: X-ray wrists: Slight improvement in rachitic changes which, however, are still gross.

Case II.—G. J., girl, aged 5 years.

Past history.—Admitted to hospital at 2¼ years for investigation of glycosuria, thirst, enuresis, increasing constipation. Noted as thin, pale, listless, with bossing of forehead. Hard fæces palpable on abdominal examination. Height 33 in. weight 22 lb. X-ray: Pituitary fossa normal. Bones not X-rayed.

Blood chemistry: "Lag" diabetic curve: fasting blood sugar 100 mg.% rising after 35 grammes glucose to 250 mg.% at one hour, falling to 110 mg.% at two and a half hours. Glycosuria nil with blood sugar 100, faint trace at 110, and present at 150 mg.% and over. No ketosis. Serum calcium 9·5 mg.%. Serum inorganic phosphorus 5·06 mg.%. Blood urea 61 mg.%. Blood chlorides 482 mg.% (as NaCl). Hb 68%. R.B.C. 3,750,000.

Urine: S.G. 1005, pH 6·8–8: albumin nil—light cloud, no casts.

June 1951: Aged 4½ years. First seen by me at Sydenham Children's Hospital as a result of the discovery that her brother (Case I) was suffering from Fanconi syndrome. Polyuria, night enuresis, thirst, constipation and lassitude had continued.

On examination.—Small stature (height 35 in., weight 26½ lb.), infantile behaviour, fair hair, pale, beading of ribs, enlarged epiphyses, frontal bossing, genu valgum, prominent abdomen, small umbilical hernia.

Cystine crystals (few) in corneæ and conjunctivæ.

Urine: Colourless. S.G. 1005, pH 6·9–7·3. Albumin trace. No casts.

Blood chemistry: Serum calcium 8·1 mg. %. Serum inorganic phosphorus 1·2 mg. %. Serum alkaline phosphatase 30·7 King-Armstrong units. Blood urea 58 mg. %. Blood chlorides 482 mg. % (as NaCl). Alkali reserve 45·8 vol. CO_2 %. Plasma α amino-nitrogen 4·14 mg. %.

Urine: Sulkowitch test no precipitate. Paper chromatography: gross cystinuria and amino-aciduria. Cyanide nitro-prusside test: raised cystine. Cystine (mg.)/Creatinine (grammes) = 300 by polarograph.

X-ray: Gross rickets. The bone architecture of fully formed bone is normal.

Treatment.—June 1951: "Adexolin" ℔ xx b.d. X-rays (28.9.51)—rickets not improved.

October 1951: Calciferol 100,000 units daily, 1½ tablespoons t.d.s. (75 c.c.) of Albright's mixture. X-rays (13.2.52)—definite improvement in rachitic changes, which are still well marked.

Comment.—Fanconi syndrome is considered by Dent (1947, 1952) to be due to an inherited defect of reabsorption affecting primarily the proximal tubules of the kidney and involving amino acids, sugar and phosphates. He believes the defect to be inherited as a Mendelian recessive. The syndrome is characterized by dwarfism, gross rickets, hypophosphatæmia, amino-aciduria (including cystinuria), renal glycosuria, cystine deposits in body tissues, and acidosis. In spite of this acidosis, the urine is almost invariably alkaline to neutral. As renal failure ensues, the blood inorganic phosphates increase and, as a concomitant, the blood calcium falls. Hence, in Case I, tetany was the presenting symptom as in one of Fanconi's original cases (1936). Clinical examination showed rickets to be present and this was confirmed by X-ray. The next step was to exclude steatorrhœa and anomalies of the renal tract. Now that the Fanconi syndrome seemed likely Dr. Dent was approached. He clinched the diagnosis by paper chromatography and advised that the corneæ be examined for cystine crystals. It is of interest to note the normal plasma levels of α amino-nitrogen while gross amino-aciduria was present. I am greatly indebted to him for these and subsequent investigations and suggestions regarding treatment.

The sheet anchors of treatment in these cases have been vitamin D and Albright's citric acid—sod. citrate mixture which are well known to influence the bone changes and acidotic conditions respectively in the Fanconi syndrome, but is not yet known to have any material effect on the fatal outcome.

Vitamin D "Adexolin" ℔ xx b.d. for four months, together with the injection of 600,000 units of "Sterogyl", produced some improvement in the rickets in Case I, but "Adexolin" alone had hardly any effect in Case II. Calciferol 100,000 units daily for the past five months was given to both children without any ill-effects. In Case I the rickets improved a little more but in Case II the benefit was marked. These results seem to show that vitamin D must be given in very high dosage to be of any use.

Albright's mixture.—Treatment for six weeks corrected the acidemia in the boy but there is reason to believe that it was taken irregularly latterly with the result that his alkali reserve has fallen to its previous figure.

In view of the invariably fatal outcome expected in these cases, a new approach to treatment was felt necessary. Normally, cysteine is oxidized to cystine and the latter reduced to cysteine. On Dr. Dent's suggestion, ascorbic acid was tried in order to attempt to reduce the cystine deposits to cysteine, but later tocopherol (a biological anti-oxidant) was used to see if it could prevent cysteine from being oxidized to cystine.

Ascorbic acid.—1,000 mg. daily for seven weeks produced no appreciable improvement. For the last month of this treatment, "Ephynal" (tocopherol) was given as well: cystine deposits in corneæ were noted to be rather less than before.

Tocopherol ("Ephynal"—Racemic alpha-tocopherol acetate).—100 mg. daily. After six months much less cystine present in the eyes. In addition, the blood urea fell to 26 mg. % after ten weeks' treatment and now, even though acidemia has recurred, it is still only 44 mg. %. Part of this improvement may have been due to Albright's mixture but, in Dr. Dent's experience, the most this alone could do would be to halve the blood urea. The results so far achieved are biochemically gratifying but only further trials can decide whether tocopherol has a part to play in the treatment of Fanconi syndrome.

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Cat-Scratch Fever.—P. J. N. COX, B.M., M.R.C.P. (for Professor ALAN MONCRIEFF, C.B.E., M.D.).

L. H., girl, aged 8 years. Admitted 27.11.51.

History.—Two weeks ago onset of slight fever, a painless lump in the left groin, and two red spots on the left thigh. (Primary lesion.)

Three days ago developed itching red plaques on both shins. No chemotherapy had been given.

Examination on admission.—Not ill. One large soft left inguinal gland with some redness of the overlying skin. Two small painless red papules on the left thigh 4 in. above the knee and, in a similar position on the right thigh, a cat-scratch. Well-marked erythema nodosum on both shins.

Progress.—The erythema nodosum faded rapidly. The inguinal gland became fluctuant and thick greenish-yellow pus was aspirated twice in the first week. She ran a slight fever up to 99·6° F., without constitutional disturbance, for about two weeks and at the end of this period was discharged well, the inguinal gland enlarged but not fluctuant. No chemotherapy was given. During the subsequent two months she has remained well; pus has been aspirated from the gland on one further occasion, but it is now getting smaller.

Investigations.—W.B.C. 8,100, Neutros. 59%, lymphos. 41%. B.S.R. 21 mm. per hour (Westergren). Mantoux 1/100 negative. Chest X-ray normal.

Pus from gland: No organisms seen or cultured. No acid-fast bacilli or virus elementary bodies seen. Inoculation of guinea-pig and other animals, negative (Dr. J. A. Dudgeon).

Biopsy of primary lesion on thigh: A largely monocytic granuloma with some giant cells, but the picture is not that of tuberculosis.

Frei test negative.

Complement-fixation reaction for lymphogranuloma negative.

Skin test for cat-scratch fever (Mollaret, 1950) positive. An antigen prepared from this child has given positive skin reactions in other similar cases.

Comment.—Cases of cat-scratch fever have been reported with increasing frequency in the last two years, chiefly in France and America: this is the first report of a case occurring in England. It is a benign disease characterized by mild constitutional disturbance and gross localized enlargement of lymph nodes, which frequently suppurate; there is usually a small primary skin lesion, commonly originating from a cat scratch. The cat is unaffected by the disease and is presumed to be a passive carrier of the infective agent; this has not yet been isolated, but it has been suggested that it may be a virus of the lymphogranuloma-psittacosis group.

The case described here shows the typical features of the disease. The occurrence of erythema nodosum is uncommon, but has been described before. The condition is probably quite common in this country and is most likely to be mistaken for tuberculosis, or a simple pyogenic adenitis, in which the pus has been sterilized by chemotherapy.

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Congenital Partial Heart Block.—H. G. DUNN, M.R.C.P.

S. T., a girl, aged 13 months, is the second child of healthy parents. The mother was well during both pregnancies, and the first child is normal.

Nine days before the second delivery, irregularity of the fœtal heart rhythm was noted and dropped beats were demonstrated by phonocardiogram (Fig. 1).

The fœtal heart continued to beat irregularly throughout labour. The infant was delivered normally and appeared healthy. A soft systolic murmur, audible from the cardiac apex to the left lower sternal border, was noted and was best heard after dropped beats.

At the age of 4 and 8 days, electrocardiograms (ECG) showed a variable degree of partial atrioventricular block with Wenckebach periods; the P–R interval ranged from 0·20 to 0·28 second (Figs. 2 and 3). At 12 days, the ECG demonstrated 3 : 2 and 4 : 3 block at an auricular rate of 155 to 160 per minute. After a crying attack this rate rose to 215 and all beats were conducted, the P–R interval being 0·22 second. Again, twenty-five minutes after a subcutaneous injection of atropine 1/200 grain the auricular rate was 200 per minute and all beats were conducted, with a P–R interval of 0·23 second. Chest X-rays and screening with barium swallow showed nothing abnormal.

At the age of 1 month, the ECG showed 2 : 1 and 3 : 2 block at an auricular rate of 155 to 170 per minute. Compression of the left carotid sinus caused auricular slowing to 115 per minute with dropped beats after every two to five ventricular responses and finally sino-auricular block with auriculoventricular nodal escape.

During the subsequent two months, the heart showed variable degrees of block. At the age of 3 months it was beating regularly at a rate of 70 per minute, and the ECG demonstrated 2 : 1 block with a P–R interval of 0·26 second.

At *nearly 5 months*, 2 : 1 block was again shown electrocardiographically and now proved persistent thirty minutes after a subcutaneous injection of 1/100 grain of atropine, although the auricular rate rose from 140 to 194 per minute and the P–R interval was reduced from 0·28 to 0·26 second.
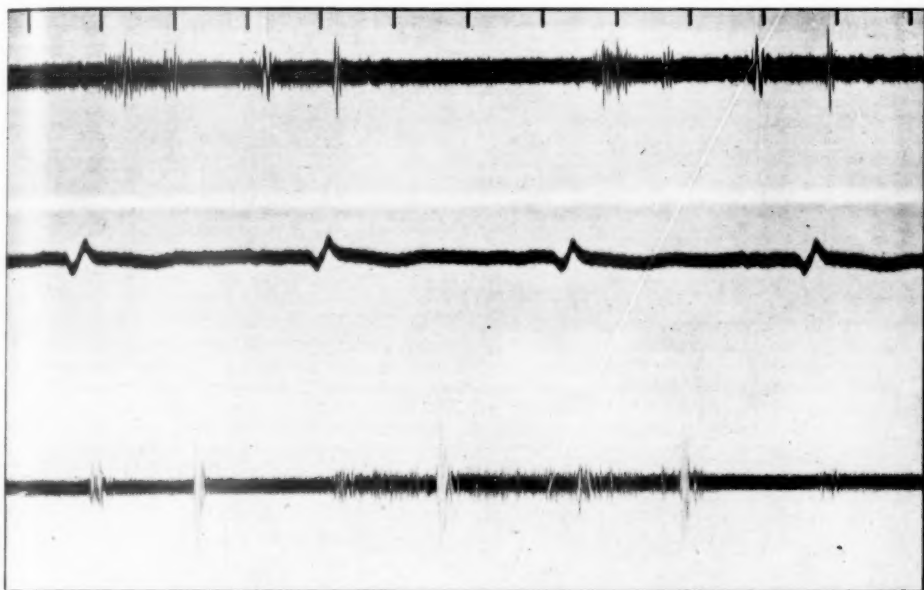
FIG. 1.—Cardiograms taken nine days before delivery. Upper tracing: fœtal phonocardiogram (medium frequency) recorded from the mother's abdomen. Note dropped beat (probably due to 3 : 2 heart block) and systolic murmur during subsequent contraction. Middle tracing: electrocardiogram from a lead connecting fundus uteri and mother's left leg. Lower tracing: maternal phonocardiogram (high frequency) recorded from pulmonary area. The main excursions in the electrocardiogram are maternal and coincide with the mother's first heart sounds. Time marker = 0·2 second.
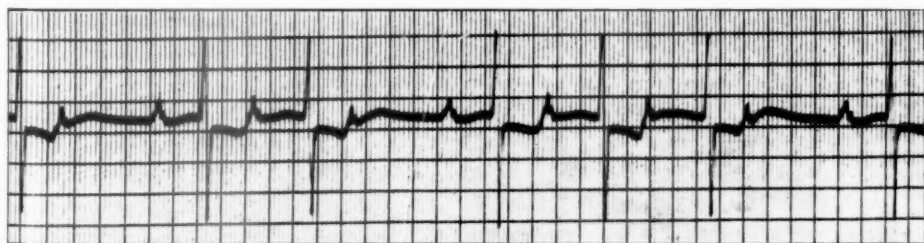
FIG. 2.—Electrocardiogram (lead CR₁) taken four days after birth and showing 3 : 2 and 4 : 3 heart block with Wenckebach periods. P–R interval = 0·20 to 0·26 second.

The child has developed normally and looks healthy. Her pulse-rate is usually about 70 per minute. B.P. 115/65. The systolic murmur remains soft, is now best heard at the apex and is not diagnostic of any congenital lesion. Slight general cardiac enlargement can be demonstrated radiologically but could be explained purely by the bradycardia. The ECG shows persistent 2 : 1 block and the P–R interval has now lengthened to 0·30 second.

Comment.—Congenital partial heart block is rarer than congenital complete block, but a complete block may sometimes develop from a partial one. The progressive increase in the degree of block in the present case is of interest in this connexion.
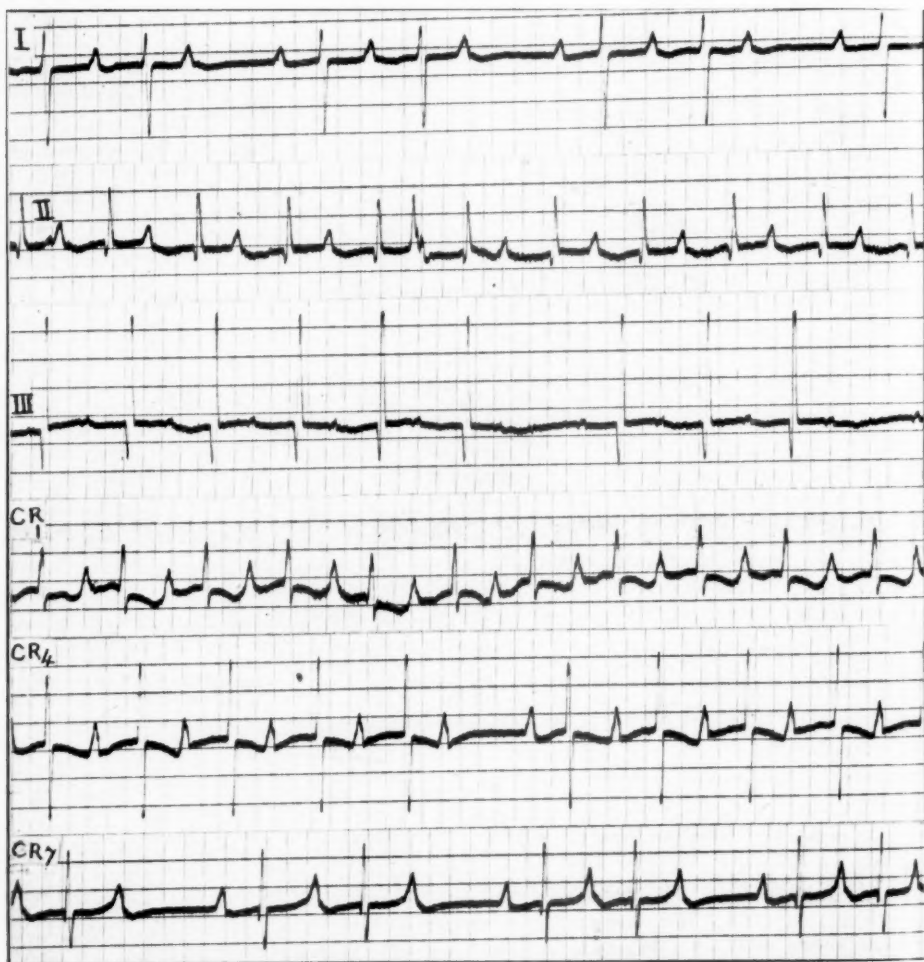
FIG. 3.—Electrocardiograms (limb and chest leads) taken eight days after birth and showing partial heart block. The degree of conduction defect varies from only prolonged P–R interval (leads II and CR$_1$) to 3 : 2 block (leads I and CR$_7$). Note Wenckebach periods. P–R interval = 0.22 to 0.28 second.

In a few recorded cases congenital complete heart block has been demonstrated phonocardiographically before birth. The present appears to be the first instance of a partial block thus documented.

I am grateful to Dr. K. H. Tallerman for permission to publish this case and to Dr. William Evans for allowing me to reproduce the cardiograms, which were recorded by the Staff of the Cardiac Department of the London Hospital.

Section of Dermatology

President—G. B. DOWLING, M.D., F.R.C.P.

[*February* 21, 1952]

An Epidemic Follicular Eruption

By R. E. BOWERS, B.Sc., M.D., M.R.C.P.

THE object of this communication is to give a preliminary account of an epidemic follicular eruption which appeared in Cheltenham early in January 1952.

Clinical picture.—The condition varies little from case to case in its essential features. The majority of patients are children and young adults, but no age is exempt; the extremes so far are ten weeks and seventy years. Both sexes are affected; there is an apparent excess incidence in young females.

They rarely suffer any constitutional upset, and the rash appears suddenly, sometimes attended by itching, which is never severe. Some of the children vomit once at the onset; a few have a slight fever—not above 100° F.—and a generalized lymphadenopathy, but these findings are exceptional. The spleen has not been felt in any patient.

Examination in the early stages reveals a papular erythematous follicular eruption. In most patients the face and back are affected, particularly the forehead and mastoid areas; the arms are quite frequently involved, the abdomen occasionally, and the legs seldom (Figs. 1 and 2).

FIG. 1.—Girl aged 19, second day of disease.
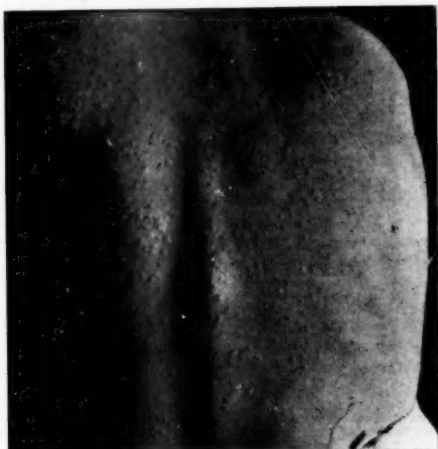
FIG. 2.—Girl aged 6, fourth day of disease.

Some of the lesions show comedo formation in the early stages; three or four days later all patients show a variable degree of horny plugging in the follicle mouths; the plugs protrude, particularly on the back, and give a true nutmeg-grater feel to the skin.

After a time varying from four to ten days, the skin returns to its original state, leaving no sequelæ. So far, 3 patients have had second attacks.

White-cell counts have been done in 6 cases. All showed an eosinophilia of 2–6%, but the differential counts were otherwise normal. 4 had a mild leucopenia, but the total white-cell counts were done on oxalated blood and may not be reliable. Five counts were repeated on fresh blood after a week: the totals were normal, but the mild eosinophilia persisted.

Pathology.—Sections taken from 2 patients on the second and third days of the rash respectively, show collagenous œdema, maximal around the follicles in 1 patient, but generalized in the other.

Dr. Ian Whimster commented that the appearance was that of an urticaria and suggested that it might be due to an antigen entering the skin through the follicles. He thought that the horny plugging noted in the later stages of the eruption was secondary to the perifollicular œdema.

Dr. Alastair Dudgeon kindly stained and examined some sections for me, and did not think that the appearance was compatible with a virus infection.

Egg culture (Dr. F. O. MacCallum, 12.4.52): Several passages on chorioallantoic membrane of 10–12 day embryos gave no specific results either macroscopically or microscopically.

Epidemiology.—The outbreak appeared about January 7, 1951, and was reported to the Medical Officer of Health as a new epidemic by Drs. J. R. Miles and F. S. Mellows of Cheltenham. In the first ten days these practitioners and their partners saw and recorded about 50 cases. Enquiry of other practitioners revealed a varying incidence, and at a very rough estimate there must have been at least 200 cases in the town.

New cases soon became very scarce, and the last was affected on January 14, by which time nearly all the other patients were well on the road to recovery.

There was then a lull, broken suddenly on February 3, when 5 cases appeared in one house of a large school for girls. By February 11, 22 out of 44 girls in that house were affected; and it had appeared in 14 girls, a housemistress and the cook of another house. There was an incidence of 0 to approximately 25% in other houses, and the most recent figures show 80 cases out of 1,000 girls in all. One or two have been sub-febrile for a day or two at the onset.

More cases were reported from the town after February 4, but the epidemic is once again dying away, and I could find no patient to show to the meeting to-day.

Geographical incidence.—Cheltenham (population 65,000) is the only town affected in North Gloucestershire. The City of Gloucester (64,000) and the towns of Tewkesbury (5,000) and Stroud (16,000)—all within about 12 miles—have produced no cases.

Cheltenham itself is divided into approximately two halves by the High Street, running East to West. At first most of the cases were thought to be North of the High Street, but recently many have appeared in the Southern portion, which is on the whole less thickly populated and includes the girls' school mentioned above.

Two large schools for boys have shown no cases among boarders, and so far only one in a day-boy.

Causes.—The broad possibilities in this epidemic seem to be three: (1) An infection. (2) An irritant applied externally. (3) A toxin, ingested or inhaled.

Infection: If this is the cause, the incubation period would be that between the two waves, namely about three weeks; but it is very hard to explain: (*a*) The absence of cases in near-by town or villages. (*b*) The different attack-rate in different houses of the same school. For instance, House X has 50% incidence; House Y close at hand with the same type of population at risk and mixing freely with House X in school, has no cases. Other houses have a varying number of cases, but all appeared within a week or ten days.

It is necessary in any event to suspect the water supply as a possible vector of infection, toxins or irritants, and in this connexion it is possible to divide the town into two areas—A and B. Area A, comprising about a quarter of the town, receives its supply solely from source A. Area B, the rest of the town, receives water from source A in varying amounts, but also receives it from a second source B.

Source A also supplies water to Tewkesbury and some parts of Gloucester; source B supplies only Cheltenham.

It is possibly significant that all cases so far have occurred in area B; none in areas supplied by source A alone.

All the water is chlorinated, and is usually bacteriologically sterile. Some derivative of chlorine is a possible cause, but it is strange that no such epidemic ever arose as a result of heavily chlorinated Army water.

Once again, the grossly different incidence in neighbouring houses of the girls' school on the same water supply, makes water an unlikely cause.

[POSTSCRIPT.—House Y with no cases, has subsequently been found to be on the very edge of the suspected supply, and at times may receive water only from source A, which is not under suspicion.]

External irritants: These include water—again unlikely, and soaps used either at home or at laundries. Soaps used at home will not be the cause, in view of the explosive onset in nearly all parts of the town; and inquiry shows that a number of the patients do not patronize laundries.

Again, in the earliest stages, the horny layer shows no sign of damage.

Toxins, including allergens: (*a*) Inhaled toxins: These can be ruled out because of the wide distribution of cases.

(*b*) Ingested toxins: It is possible that detailed studies of the diets and shopping habits may reveal some common factors. Such studies are being carried out at present by the Medical Officer of Health, particularly in the girls' school where the population is under close observation.

In possible support of the idea that foodstuffs are responsible is the fact that only one case has been seen in an infant at the breast or bottle-fed: one would have expected such infants to be particularly susceptible to infections or external irritants.

Comment.—Early in the epidemic, Dr. G. B. Dowling to whom I am very grateful for much help, and who has examined a number of the patients, drew my attention to the paper by Schuppli (1947), in which he reported an outbreak of follicular keratosis in Basle in 1946.

Comparison of Schuppli's description and photographs with the patients affected in the present outbreak makes it seem probable that we are dealing with the same entity: the only differences are as follows:

(1) Schuppli's cases seem often to have lasted more than two months: with one or two exceptions, ours have cleared in ten days at the most.

(2) Schuppli reported some peculiar variants in the later stages of the epidemic; these have not yet been seen here.

Schuppli discussed the possible relationship of his epidemic to those reported by Brooke in 1893, Rocamora in 1922 and Gougerot *et al.* in Paris in 1944, and it would seem that the later epidemics may well correspond one with the other.

In spite of exhaustive enquiries, Schuppli was unable to find a cause for his epidemic, which lasted through much of the year 1946, and died away in the autumn.

POSTSCRIPT.—March 30, 1952: Dr. Morley, Medical Officer of Health, Cheltenham, informs me that a certain reservoir, forming part of source B, was brought into action after three months' disuse on January 5. The subsequent course of events was as follows:

Reservoir on January 5. First cases January 7. Off January 13. Last cases January 14. On February 1. More cases February 3. Off February 15. Last cases February 17.

It was turned on again from March 3 to 23, and 6 cases—some of them relapses—were reported from the girls' school on March 7, 8 and 10, but I am only aware of one outside the school, who was affected on March 7.

It therefore seems reasonable to suspect the reservoir in question of brewing some allergen, and that the latter has now become diluted, or the population immune.

Dr. D. S. Wilkinson has made the interesting suggestion that this might be a cercarial dermatitis, as described by Cort (1950). The reservoir concerned has been carefully examined and cannot support any snails to act as intermediate hosts to cercariæ.

I wish to thank Dr. A. W. Mucklow and the general practitioners of Cheltenham for their co-operation; Dr. E. N. Davey and Dr. A. B. Gardner for the blood counts; Brigadier A. E. Richmond of the Ministry of Health for his interest and advice, and Dr. Donald Morley, Medical Officer of Health, Cheltenham, for much of the epidemiological information in this paper, and for the photographs.

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The President: We are grateful to Dr. Bowers for bringing this very interesting epidemic to our notice. He kindly asked me to see some of the cases, I was very glad to do so, but I have nothing to add to his remarks.

Dr. R. T. Brain: Was there any question of swimming bath contamination? I have seen children with a marked follicular keratosis in the summer which I related to chlorinated bath water.

Dr. John Savage: I was talking to Dr. Ferguson Smith, who had to leave before the discussion. He said he remembered the Basle cases and he understood the cause was proved to be chlorine-impregnated grease-proof food-wrapping paper.

The President: Was it suggested that this was externally produced or that the food was contaminated in some way?

Dr. John Savage: He did not say definitely but I gathered it was the chlorine-contaminated food which caused the trouble.

Dr. R. E. Bowers: I have not inquired about the swimming bath but I do not think the Girls' School does swimming at this time of the year. Furthermore I have seen an affected infant of only ten weeks. With regard to Dr. Savage's suggestion of grease-proof wrapping paper I will make inquiries. I have not been into that aspect.

POSTSCRIPT (24.6.52).—Further inquiry has not supported either of these suggestions.—R. E. B.

Carcinoma Erysipelatodes.—N. A. THORNE, M.D., M.R.C.P. (for W. J. O'DONOVAN, M.D.).

Mr. C. L., aged 81, states that his general health has been good in spite of operations for removal of adenoma of the prostate and repair of an inguinal hernia nineteen years ago, and the occurrence eight years ago of a gastric ulcer. He gives no history of injury to the scalp but about twenty years ago he first noticed a small swelling on the right anterior part of the scalp which gradually increased in size, and in its earlier stages was apparently translucent. Fourteen years ago it broke down discharging blood and "coffee material", and this has happened several times since. Two years ago the swelling was lanced but has since slowly increased in size. Following the shock of his daughter's sudden death, eight months ago, he observed a pink discoloration on his forehead which has gradually spread downwards on to his face. He has experienced a burning sensation at times over his face and stiffness of the eyelids. There has been some loss of weight during the past year.

Family history.—One son has died of a pathological fracture of the leg due to secondary neoplasm and a daughter has died following an operation for an "ulcer of the rectum".

On examination.—There is a tumour on the right anterior parietal region of the scalp measuring 3·5 × 3·5 × 1·6 cm. It is of irregular outline, firm but slightly fluctuant, not attached to the underlying bone, and covered with normal skin except on its right margin where there is a superficial ulcer spreading on to the scalp, measuring 1·3 × 2·6 cm. Over the left vertex and passing backwards towards the occiput, there is an S-shaped infiltrated erythematous band on which a few hairs are growing.

Spreading down over the face from the upper forehead as far as the neck, but sparing the distal one-third of the nose, the upper lip and the area below a line drawn from each angle of the mouth to the corresponding angle of the jaw, is an erythematous eruption with a definite circinate and slightly raised margin. There is infiltration of the affected skin, especially the eyelids, which are prevented from opening fully. Eyelashes are still present. Over the affected region are several areas of normal skin, the largest being above the left eyebrow. Telangiectases are also visible.

The lymph glands of both posterior triangles are enlarged, some having already become matted together. No lymphadenopathy can be detected elsewhere. Apart from some seborrhœic warts, the rest of the skin is normal. General examination fails to reveal any gross physical signs.

Investigations.—Blood count within normal limits. Blood urea 45 mg. per 100 ml. Blood Wassermann negative. Urine normal. X-ray chest normal. Barium meal: slight irregularity of posterior aspect of the lesser curve of the stomach but no ulcer seen.

Histology (Dr. D. J. O'Brien).—(*a*) Piece of trabeculated polygonal cell carcinoma, probably squamous carcinoma, from flat lesion of scalp. (*b*) Small group of polygonal carcinoma cells in lymphatics of dermis of portion of skin from face.

Comment.—The first reported case is that of Küttner (1924) who named the condition Erysipelas carcinomatosum, a term since discarded as being genetically incorrect. The name Carcinoma erysipelatodes was first used by Rasch (1931). In the English literature 6 cases are recorded, 1 each by Parkes Weber (1933), and Vonno (1933), who both favour the term Carcinoma telangiectaticum, 2 by Gordon (1932 and 1937), and one each by Goldsmith (1929), and Barber (1932).

It is interesting that all were female patients and that the condition was either associated with a primary carcinoma of the breast or with Paget's disease of the nipple. In all but one case the skin manifestations occurred close to the primary growth—the exception being Goldsmith's case where blood-borne metastases appeared in the skin of the scalp.

In the present patient no primary neoplasm could be found in the common visceral sites, e.g. breast, stomach, lung or colon, so that it would appear that the primary must have developed in the wen or atheroma which has been present for some twenty years.

Although Shattock (1897) pointed out that the common wen was a cyst, arising in most cases from a hair follicle and not from a sebaceous gland, a confusion of terms has since existed in the literature. Love and Montgomery (1943), in their paper on epithelial cysts, studied 271 cases seen in the Mayo Clinic during 1939–40. Histological examination was performed in 88 cases showing 51 to be epithelial cysts of which 2 were of traumatic origin. 4 cases (1·5% of the total), showed malignant changes: 2 of these were basal-cell carcinomata arising from sebaceous cysts and 2 were squamous-cell carcinomata arising from true epithelial cysts. They concluded that trauma and irritation played a minor role in the development of malignant changes.

Caylor (1925) reviewed 236 cases of sebaceous cysts without making the distinction between these and epithelial cysts and stated that 3·4% showed malignant changes. Other American authorities have produced even higher rates of malignancy but similar figures have not been recorded amongst those cases reviewed in this country.

It is my opinion that in the present case an epithelial cyst, having been present for some twenty years, has undergone malignant changes as shown by the trabeculated polygonal carcinoma cells found at biopsy. Neoplastic cells have then spread by the lymphatics to glands in the neck and to the skin of the greater part of the face; further invasion of the blood capillaries has given rise to telangiectases.

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Parapsoriasis-en-plaques, with Retiform Lesions.—E. WADDINGTON, M.D., M.R.C.P.

Mrs. H. G., aged 47.

Eighteen years ago she noticed a non-irritating scaly patch on the right buttock. Since then, similar scaly areas have developed on the buttocks, thighs, trunk and arms. The most recent lesion appeared a year ago at the back of the left thigh. The patches have never disappeared completely, although temporary improvement followed a course of ultraviolet light.

She first attended hospital eighteen months ago, because of increasing irritation on the buttocks, and she had also noticed that these lesions had become brighter in colour during the last few months.

On examination.—There are numerous erythemato-squamous plaques on the trunk and limbs. The face, hands and feet are spared. The majority of the plaques are yellow in colour, and the surface is covered with small adherent scales. They are of varying size and irregular shape, and coalescence of the plaques has produced polycyclic lesions. There is no infiltration. On the upper part of the left thigh there is a retiform lesion, the meshes are erythematous and the skin is atrophic. On the buttocks the lesions are similar. There is telangiectasia and adherent scaling, and the skin is pleated like cigarette paper, although there is no atrophy.

Biopsy (Dr. I. Whimster) *from the right buttock shows:*

Epidermis: Slightly hyperkeratotic and hyperplastic. The basal layer is largely depigmented, and contains many vacuolated cells.

Dermis: Occupying the upper third, there is a band of inflammatory cells and very numerous melanophores, and in this region the connective tissues are degenerate.

Comment.—The majority of the lesions are typical of Brocq's parapsoriasis-en-plaques, but on the buttocks and upper part of the left thigh they are of the rare retiform variety. These lesions closely resemble poikilodermia atrophicans vasculare, although there are no lichenoid papules characteristic of that condition. The patches on the buttocks and thigh remained unaltered for over sixteen years, and then the patient noticed the colour change, and complained of increasing irritation. These symptoms may be significant.

Lapière (1948) attempted to distinguish between the parapsoriasis of Brocq and the clinically similar premycotic erythema. He was able to show that there are certain features which favour a diagnosis of a premycosis, and among these he mentions the development of a reticulated appearance and the onset of pruritus. Keil (1938a and b) reviewed the literature, and came to the conclusion that Brocq's parapsoriasis-en-plaques, and the retiform type with its transitions into poikilodermia represent phases of a single condition that terminates in mycosis fungoides.

In view of Lapière's work, I think this is a premycosis, and it seems probable that the lesions will first develop into a poikilodermia atrophicans vasculare before their final evolution into mycosis fungoides.

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Dr. C. H. Whittle: We have a man aged 32 with four years' history of parapsoriasis of this type. There are scaly sheets of erythema on the trunk and thighs, with atrophy and retiform changes. The histopathology gives a picture resembling lichen planus. Treatment with thorium-X painting has resulted in complete resolution of the treated lesions, with no change in the untreated ones.

The following cases were also shown:

Keratodermia Blennorrhagica.—Dr. D. S. WILKINSON.

Molluscum Sebaceum (Kerato-acanthoma, Benign Epithelioma). Two Cases.—Dr. C. H. WHITTLE and Dr. A. LYELL.

Neurofibromatosis of Recent Development.—Dr. R. G. HOWELL (for Dr. G. B. DOWLING).

Pemphigus Foliaceus.—Dr. D. I. WILLIAMS.

Atrophic Scleroderma.—Dr. C. D. CALNAN (for Dr. I. MUENDE).

Case for Diagnosis. ? Polyarteritis Nodosa.—Dr. J. G. HOLMES (for Dr. G. B. MITCHELL-HEGGS).

(These cases may be published later in the *British Journal of Dermatology*.)

Febrile, Relapsing, Nodular, Non-suppurative Panniculitis (Weber-Christian).—J. E. M. Wigley, F.R.C.P., and O. L. S. Scott, M.R.C.P.

Married woman, aged 43. Admitted to Charing Cross Hospital in February 1952, under the care of Dr. R. A. Hickling. She has suffered from vague ill-health for the past four years, which has been more noticeable in the past two years. Nine months ago she developed sharp pain in the right subcostal region, and this was followed within four weeks by the appearance of lumps on the legs. Since then other lumps have appeared on all four limbs, buttocks and chest, and they have tended to appear in crops. She has also suffered from diarrhœa during this time and fever. The former has been shown to be due to steatorrhœa. Histology of a recent subcutaneous nodule has been reported upon by Dr. H. Haber, who considered it to be consistent with a diagnosis of Weber-Christian syndrome, and drew our attention to a fatal case described by Bjornstad which had some similar features.

The disorder consists of recurring episodes of fever and the appearance of numerous tender, often painful, subcutaneous nodules. The nodules consist of non-suppurative inflammation and necrosis of the subcutaneous fat and other regions. The title "relapsing, nodular, non-suppurative panniculitis" was proposed by Dr. Parkes Weber in 1925 when he showed a case to the Association of Physicians in Belfast. In 1928 Christian rightly added the adjective "febrile" when he described his case which relapsed ten times in five years. Since then 44 cases have been reported. Its importance lies in the fact that it may pass unrecognized, may lead to severe disability, and in 6 cases it has terminated fatally.

The nodules in this case appear, as typically, in crops. At first they are only palpable and tender, later become red and visible, then darker red, brown and scaling, and finally in some cases break down and ulcerate (Fig. 1). If there is no ulceration the nodule heals with a puckered scar. This

Fig. 1.—Weber-Christian disease, showing distribution of nodules on left leg.

patient has had a low-grade fever for the past three months which rises to about 100° F. at night. She has lost about 2 st. in weight.

The interesting features of this case are the blood counts and the steatorrhœa. She has a macrocytic anæmia with moderate anisocytosis, poikilocytosis and polychromasia (Hb 79%, R.B.C. 3·6 million per c.mm., C.I. 1·09). This state of affairs has been found in several other cases and may be explained by the fatty degeneration and necrosis of the liver that has been seen in 4 cases post mortem.

Her steatorrhœa is due to failure of absorption of split fat (split fat 56, unsplit fat 8, total fat 64 grammes % of total dry weight). This symptom has not been noted in other cases, but it is probably caused by involvement of the intestinal wall and mesentery. These regions showed necrosis of fat and infiltration with macrophages in the case reported by Mostofi and Engleman (1947) at post-mortem, and our patient may be similarly but more severely affected.

Finally, the white blood count shows changes suggestive of glandular fever or lymphatic leukæmia (W.B.C. 15,100; Neutros. 30·5, lymphos. 54·5, monos. 15%). We are unable to account for this.

Treatment has not yet been started but it is proposed to try a course of sulphapyridine which proved helpful in a case described by Arnold. It appears to have acted almost specifically on five separate occasions, and the other sulphonamides proved less useful.

POSTSCRIPT (April).—The patient was given a course of sulphapyridine for five days which made her extremely ill and produced no improvement. She was then given a course of chloramphenicol: 500 mg. six-hourly for ten days followed by 250 mg. for four days. Rapid improvement coincided with the administration of the antibiotic and all the ulcers, including the large one on the thigh, were practically healed within a fortnight. Her blood count remains unaltered. It is hoped to publish fuller details later.

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Dr. H. Haber: The focus of the lesion lies within the subcutaneous tissue, which exhibits necrosis of its cells. There are characteristic oil cysts lined by foamy histiocytes which are also found lying between the fat cells. Many histiocytes have also disintegrated as seen by nuclear debris. There are also several giant cells of the foreign body type and large vacuolated endothelial cells.

A marked inflammatory reaction within the interstitial spaces of the tissue consists of plasma and round cells. The infiltrate continues along the blood vessels right up to the subepidermal part of the corium. The corium itself shows œdematous separation of its bundles which appear to be sclerosed. There are also many monocytes which exhibit different stages of differentiation to lipophages. The blood vessels show proliferation and one vessel lying within the adipose tissue is obliterated. The epidermis shows changes of an eczematous type. The diagnosis is nodular non-suppurative febrile panniculitis (Weber-Christian).

Dr. B. Schwartz: In the case I showed in December (*Proc. R. Soc. Med.,* 1952, **45,** 174) large numbers of the nodules had actually ulcerated and broken down. Chloramphenicol was used in that case, with quite remarkable results. The lesions healed completely at first and the patient remained reasonably well and ambulant subsequently. A current relapse is again being treated with chloramphenicol. It is probably worth trying here.

Acanthosis Nigricans.—O. L. S. SCOTT, M.R.C.P.

Woman, aged 52. She was perfectly well until seven months ago. After a dental extraction and fitting of new plastic dentures she developed gradually increasing pain and difficulty with eating, noticed a waxy thickening of the palms, and was troubled with a persistent lacrimation. She has lost weight, has had vague indigestion and has noticed the appearance of warty growths, pigmentation and thickening of the skin of the waist and flexures.

Biopsy of the skin from the axilla shows appearances compatible with acanthosis nigricans. Biopsy of one of the enlarged glands in the left supraclavicular fossa shows squamous-cell carcinoma and also numerous melanin-laden phagocytic cells. It is considered most likely that there is a primary bronchogenic carcinoma. No evidence of neoplasm has been found despite careful clinical investigation, barium meal, repeated X-ray of chest, and histamine test meal.

This case shows involvement of the conjunctiva and buccal mucous membrane and tongue. The main and original symptoms have been pain and difficulty with eating. These have been controlled by mouth-washes before meals with 0·05% percaine in normal saline. This has enabled her to take a high-protein, semi-solid diet which included casein hydrolysate, and massive doses of vitamin A, viz. 150,000 I.U. daily. Her general condition improved greatly. Her tongue showed the greatest change, for when first seen its surface resembled and could be parted like the pile of a carpet. Whether this change in the mucous membrane of the mouth was caused by the treatment it is impossible to say.

Another feature of this case is the presence of the melanin-laden cells in the cervical glands removed for biopsy. I have not been able to find reference to this occurring in other cases of acanthosis nigricans, and the cause is obscure.

The four most plausible theories regarding the origin of acanthosis nigricans are: firstly, involvement of the abdominal sympathetic nervous system or adrenal glands; secondly, toxæmia from neoplasms giving rise to altered response to light; thirdly, avitaminosis; and, fourthly, endocrine disturbance. None of these theories will explain all the cases. Curth, H. O. (1948, *Arch. Derm. Syph., Chicago,* **57,** 158), in the most detailed study of the literature so far, including some cases of her own, states that she believes there is no difference between the juvenile, so-called benign form and the adult, so-called malignant form of acanthosis nigricans. She postulates a common origin for both, and considers it possible that there is a genetic relation between them and cancer—particularly "gland cancer". She states that the "role of cancer in family histories of patients with acanthosis nigricans, the type of cancers that have been associated with acanthosis nigricans and the chronologic independence of the two conditions are supporting evidence".

Acanthosis Nigricans.—B. SCHWARTZ, M.D.

C. G., aged 45. Lorry driver.

Attended the Skin Department at Central Middlesex Hospital in February 1952 for removal of the multiple warts on his forearms. These were first noticed in July 1951, and had been increasing in number since. There were no subjective symptoms.

Had been successfully treated for a contact dermatitis in May 1951. The hands and forearms only were affected and the rest of the skin was clear.

30.8.51: At operation a carcinomatous growth involving the middle and lower thirds of the stomach was found. There were numerous secondaries scattered over the peritoneum and biopsy of one of these showed omental fat infiltrated by anaplastic adenocarcinoma. In addition there were three metastases in the gall-bladder but none in the liver or any other abdominal viscera. A palliative gastro-enterostomy was performed.

Family history.—Nothing significant.

On examination.—There is hyperpigmentation chiefly of the face and neck, the axillæ and the groins. On the pigmented areas are numerous warty growths of varying size with a particularly large plaque at the right wrist. The close-set lesions on the hands have given these a velvety appearance.

There are papillary growths around the mouth and extending on to the mucous membrane and also on the sides of the tongue. There is no excess pigmentation in the mouth.

Blood count normal. B.P. 124/80.

He has gained 4 st. in weight since his abdominal operation and is now completely free from gastric symptoms.

Histology (Dr. H. Haber).—(1) Skin from right axilla shows typical acanthosis nigricans. (2) Skin from right wrist shows verrucose hyperplasia with highly pigmented basal-cell layer.

Comment.—This is, I think, a classical case of acanthosis nigricans.

Two points in this case are worthy of record.

Firstly, because of his previous attendances in the Skin Clinic one can accurately place the time of onset of the skin condition between June and August 1951, i.e. at a time when his abdominal malignancy must have been well advanced.

Secondly, a gastroscopic examination was made by Dr. Avery Jones on 27.2.52, when the stomach was seen to be rigid and not distensible and the antrum almost completely obstructed. There was no ulceration of the stoma and no evidence of any papilliferous growths.

In the past most of these cases have been associated with a glandular type of carcinoma. It would therefore be interesting to know what the final pathological diagnosis is in Dr. O. L. S. Scott's case.

The following cases were also shown:

Kaposi's Multiple Pigmented Hæmorrhagic Sarcoma.—Dr. M. GARRETTS (for Dr. W. N. GOLDSMITH).

Kaposi's Idiopathic Multiple Hæmorrhagic Sarcoma.—Dr. P. J. HARE.

Two Cases of Chronic Lymphœdema.—Lieutenant-Colonel K. GREENWOOD, R.A.M.C.

Cutis Verticis Gyrata.—Dr. PETER BORRIE.

Pemphigus Vegetans (Neumann).—Dr. IAN MARTIN-SCOTT.

Lichen Striatus.—Dr. R. H. SEVILLE (for Dr. R. M. B. MACKENNA).

(1) Juvenile Xanthomata. (2) Atrophie Blanche des Jambes.—Dr. C. D. CALNAN.

Atrophie Blanche.—Dr. R. H. MEARA.

Pityriasis Lichenoides et Varioliformis.—Dr. K. D. CROW.

(These cases may be published later in the *British Journal of Dermatology*.)

[*April* 17, 1952]

The following cases were shown:

Dermatofibrosarcoma Protuberans.—Dr. A. PORTER and Dr. H. HABER.

Sclerodermia Dermatomyositis (Electronmicrographs were also shown).—Dr. R. H. SEVILLE.

Cutis Verticis Gyrata.—Dr. J. G. HOLMES (for Dr. G. B. MITCHELL-HEGGS).

Fixed Erythema Associated with Mediastinal Obstruction.—Dr. HAROLD WILSON.

Lupus Vulgaris, Untreated for Over 30 Years.—Dr. N. A. THORNE (for Dr. W. J. O'DONOVAN).

(These cases may be published in the *British Journal of Dermatology*.)

Section of Epidemiology and State Medicine

President—Professor ROBERT CRUICKSHANK, M.D., F.R.C.P., D.P.H.

[*March* 21, 1952]

DISCUSSION: HEALTH. ITS STUDY AND CULTURE IN THE NATION TO-DAY

Sir Alan Rook (Senior Health Officer, Cambridge University): Health is of such concern to us all that it has become the formula of our usual greeting. Again when we wish to honour someone we drink their health or wish them health and happiness, putting the health first for happiness is largely dependent upon it.

Health may be likened to water, the supply of which is accepted as a right, and it is not until it has disappeared and further supplies are lacking that its value is so sharply realized.

The Constitution of the World Health Organisation defines health as a "state of complete physical, mental and social well-being and not merely the absence of disease or infirmity". This is followed by the statement that "the highest attainable standard of health is one of the fundamental rights of every human being without distinction of race, religion, political belief, economic or social condition". The definition is somewhat idealistic and the statement as things are to-day is only a pious expression of faith, a vision of a state that is at present quite unattainable.

In this country in recent years far too much emphasis has been placed on ill-health and far too little on health. In other countries, prevention of disease is given a higher priority than it is here.

The preventive approach to a problem differs from the clinical approach and the training and method of practice of many doctors are such as to hinder them from obtaining a sound knowledge of preventive medicine and its potentialities. More teaching of preventive methods is required not only to doctors and medical students but by doctors to laymen, and everyone must be made to realize that health is worth paying for and with a little effort is within the reach of most. The more these methods flourish the less money need be spent on hospitals, consultants and drugs.

Hence it is disturbing to see signs of decay in the fabric of Public Health built up so laboriously before the appointed day. Local interest in health matters is waning and the M.O.H. shorn of much of his powers can only gaze impotently on the wreckage. Of every £10 of money expended on the Health Service we are told that only 3s. goes towards prevention, yet much of the ill-health of to-day is preventable. We still endure dirty food with dirty methods of storage and of transportation; we still permit dirty kitchens with large fly populations; we still tolerate filthy places of work and of assembly often ill-lit and poorly ventilated. The condition of the kitchens of certain luxury hotels requisitioned for military purposes in the last war was so deplorable that the officers who had to take them over refused to do so until they had been completely overhauled. The number of members of the Women's Services found during the war to be infected with head lice was a revelation and it is depressing to hear some years after the war has ended that nearly half a million school children in England and Wales were found on medical inspection to be verminous.

The fact that a great deal of ill-health is the fault of the individual is usually glossed over and many make little attempt to obey even the most elementary rules of health. It is true that the Government is doing its best to make over-eating, over-drinking and over-smoking uneconomical, and with a modicum of success, but there is a large part of the population that has no idea how to spend its money wisely or how best to use the leisure hours that are forced upon it. Unfortunately many members of the public are apt to resent advice which calls for some small sacrifice or which needs some effort on their part. In a future Utopian state certain forms of ill-health will be frowned upon officially and may well become the cause for a fine. I would like to suggest this to the Minister of Health as one way of helping to pay for the Health Service.

While our definition tells us that health is not a single entity but must be divided into at least three components, physical, mental and social, there is no definite boundary between these parts and although they may be discussed separately the interrelationship is so intimate that division is artificial.

For full physical health we require certain environmental conditions of temperature, oxygen pressure and the like. Given these conditions then physical health means the harmonious working of the body processes, of many of which the individual should be quite unaware, and which permits him or her to carry out a usual daily routine without hindrance or without undue fatigue.

An essential detail in the assessment of physical health is to have a working knowledge of the bounds of normality and of some method of ascertaining when these bounds have been overstepped. To wait for the onset of symptoms will not allow us to detect the slight deviations from normal which we can hope to rectify before irreparable harm has been done.

In judging physical health, particularly when choosing tests which may help the clinician, it is essential to know what form of physical exercise the examinee proposes to do, for only then can an appropriate examination be made. The man working in a London office who comes for a routine estimation of physical fitness cannot be expected to perform the same tests as those applied, say, to men about to go on an expedition to Mount Everest. This may sound elementary but it is not so long ago that doctors were trying to apply to cardiac invalids tests designed for fit young R.A.F. pilots.

Of one thing I am sure. The range of normality is wide and considerable latitude is necessary in assessing the results of physical efficiency tests in individuals who show no evidence of organic disease.

The mentally healthy may be defined as those people who, without difficulty, have weathered the various stresses which occur at different stages of life, in infancy, in childhood, at puberty and in adult life. They have an insight into their own characters and an ability to adapt themselves without friction to most of the varying conditions of human existence which they are likely to meet. They have a sense of responsibility to their fellow men, a satisfactory work capacity and the judgment to take proper advantage of their leisure. The mentally healthy may differ in their affective qualities, in the facility to absorb knowledge, in their ambitions or in innumerable other ways, yet they fall during the whole or the greater part of their lives within certain limits which permit them to be regarded by their fellows as normal or average people.

Of the prevalence of mental ill-health there can be no doubt. We are told that of 15 million men examined for military service in America $2\frac{1}{2}$ million were rejected because of neuropsychiatric disease, and that 3 out of every 7 beds in American hospitals are occupied by the mentally ill.

In this country the figures are little better, with one-quarter of all invaliding from the Services during the war the result of neuropsychiatric disorders and 2 out of every 5 hospital beds being devoted to patients with mental illness.

This means that besides the frank cases of mental ill-health there must be a vast number of borderline cases throughout the social fabric of all highly civilized countries.

Mental ill-health can almost be classed as an occupational disease among high-grade brain workers and it is one which often has a sadly deleterious effect on mental output. Among such people it tends to appear sporadically except in times of special stress; the immediate cause is often frustration and overwork, and prevention lies in recognition of the need of proper mental relaxation. The mental stresses placed on undergraduates have increased in recent years, partly as a result of attempts to ensure a fairer division of opportunity. Some with poor backgrounds and inadequate abilities are being forced forward by ambitious parents or schoolmasters, others find difficulty in adjusting themselves to new standards of living, while to a majority failure at the University is more to be feared than it was in the comparatively carefree days of twenty years ago. These stresses may become manifest in a number of ways but it is essential to realize their existence and to deal with them quickly before they become a drag on efficiency.

Despite the vast amount of work on mental tests of one sort or another which has been done during the last few years there seems to be no general agreement as to their value. Intelligence tests have earned for themselves a place in the clinician's armamentarium but intelligence and mental health are not the same thing. The so-called personality tests are probably more appropriate for the purpose but these tests are less objective and seem to have a distinctly smaller value.

It should, I imagine, be a *sine qua non* that none but the mentally fit should be permitted to apply tests to others though few of us should be sufficiently optimistic to imagine his own mental health above suspicion. It might be argued that if the physically unfit are capable of applying physical efficiency tests why should not the mentally unfit apply tests of mental fitness to their fellow men? Somehow this argument does not seem entirely sound and it does not require great powers of observation to note that the mentally blind are often only too willing to lead the mentally blind. If tolerance and moderation are necessary in assessing the results of tests for physical efficiency then they are even more essential for assessing tests of mental health. Actually it seems doubtful if mental health is a suitable subject for laboratory diagnosis or if any battery of personality tests will ever be able to supersede the sympathetic interview.

Social well-being I take to mean the presence of those environmental conditions which permit the happiness of the individual in his home life, in his work and his leisure and those conditions which foster a correct attitude to his neighbours and to society in general. A realization of the importance of social well-being is no new feature of medicine. It is the fashion to scoff at the doctor of fifty years

ago with his frock coat and his bedside manner but his intimate knowledge of his patients and their doings often allowed him to do more for them than the modern doctor with his sulphonamide drugs and his antibiotics can do to-day.

The assessment of social well-being can best be accomplished for the individual by the interview, while for the group it must usually be approached obliquely by studies of such matters as work and play achievements on the one hand and from sickness and absence rates on the other.

In our insistence on the importance of health we must guard against the ever-present danger of ill-health caused by health propaganda campaigns and the possibilities of routine examinations driving the examinee into functional disease. The health doctor must have an optimistic outlook; he must have an abounding faith in Nature, in the importance of fresh air, good food and adequate exercise combined with a sound distrust of drugs. He must not be vacillating and when he can find no evidence of organic disease in someone he has examined he should say so and say so emphatically.

There is much to learn about physical and still more about mental and social health but if this country is to hold any position in the world it is necessary to keep the ideal of perfect health continuously before us. It is important for us to know more about the factors upon which perfect health rests and it is essential for us to preach the doctrine so that everyone can strive to attain the ideal.

Dr. T. A. Lloyd Davies: *Industrial health.*—The study of health requires assessments of personality and the comparison of assessments with validated standards. In the present state of knowledge, both means of assessment and validated standards are lacking.

Young persons working in industry and commerce form the social and age group to which, in the main, my remarks will refer. During adolescence spent in gainful employment, health is the attainment of a balance, proper to the individual, between body, mind and spirit. Health is an individual matter and bears no relationship to an arbitrary "normal" or group mean. As adolescence is a time of change, standards of judgment must vary according to age, maturity and circumstances.

Growth and physical state.—Chronological age is an inadequate criterion in judging physical growth. Ellis (1946) has shown that height and weight of boys age 11–16 years is affected significantly by sexual maturity. A similar effect in girls is less well known. Girls who menstruate early are heavier at all ages from 14–18 years and approach their maximum height earlier. Late menstruating girls achieve the same height but not weight by 18 years of age.

A full account of this study is to be published shortly.

Most entrants to industry suffer from minor ailments; few have none and very few have disabilities interfering with employment (Table I).

TABLE I.—PHYSICAL STATE OF YOUNG PERSONS AGED 15–18 YEARS APPLYING FOR EMPLOYMENT IN LARGE INDUSTRIAL AND COMMERCIAL ORGANIZATION (1948–1950)

	Boys (309)		Girls (1,835)	
	No.	%	No.	%
No defects (fit for all work)	36	11	172	9
Minor correctable defects (fit for all work), e.g. caries, refractive errors, flat feet	252	82	1,586	86
Major defects not easily correctable (permitting restricted employment), e.g. monocular blindness, squint, chronic otitis media	21	7	76	4
Major defects, unfit for work, e.g. tuberculosis, epilepsy..	0	0	1*	0·05

*Pulmonary tuberculosis.

Intellect and unconscious motivation.—The outstanding mental characteristic of adolescence is a sense of uniqueness. For many, adolescence is an intensely unhappy period due to causes arising within the individual. Intensification of religious interest, anxiety over timid love affairs, unsureness of self all seem devastatingly personal. Mild schizoid symptoms are common.

Young persons in industry of less favoured attainment than university students exhibit little or no insight, have difficulty in verbalizing their problems and may, consequently, find solution or compensation more difficult. They are apt to drift into careless irresponsibility, reduce their dealing with authority to the minimum and this, combined with their need for social approval, creates further personal and social problems.

Social adjustment.—72% of boys and 83% of girls joining one industrial and commercial organization did so for social reasons (Table II).

As the inherent value of jobs decreases, so the importance of social conformity and acceptance increases. Persons working in industry are often ill-equipped intellectually and educationally to withstand its pressures. Many in industry work as a social habit or to earn a living. Satisfactions

cannot be fulfilled through the job and must be of a social character. Even tension reduction, one measure of a successful personality (Murray and Kluckholm, 1948), is determined socially, for example, by the factory whistle for dinner.

TABLE II.— REASONS GIVEN BY BOYS AND GIRLS AGED 15–18 YEARS FOR JOINING INDUSTRIAL AND COMMERCIAL ORGANIZATION 1948–1950

		Boys (162) %	Girls (872) %
Social	Friends at work	6 ⎱	24 ⎱
	Relatives at work	26	21
	Good repute	19	18
	Conditions of work	4 ⎰ 72	12 ⎰ 83
	Advertisement	0	6
	Unsatisfied ⎱ Job preference*	17 ⎰	2 ⎰
	Satisfied	15	8
	Sent by Labour Exchange ⎱	2	5
	No reason		
	Opportunity for promotion	4	3
	Security	6	0·3
	"Health" Near home	1	0·7

*Job preference includes adolescents who expressed a desire for a particular form of work. Those who achieved this desire are classified as satisfied, others who did not achieve it and, in fact, joined for social reasons are classified as unsatisfied.

The difference between job preferences of men and women is important (Archibald and Whitfield, 1947). At all ages, men rank interesting work, opportunity of promotion and responsibility very high (Table III). To a marked degree, women reject responsibility. This means that women are

TABLE III.—DIFFERENCES BETWEEN MEN AND WOMEN AND BETWEEN AGE GROUPS IN JUDGMENTS OF RELATIVE IMPORTANCE

MEN

14–17 years	18–34 years	35–64 years
Opportunity for promotion	Opportunity for promotion	Opportunity for promotion
Interesting work	Responsibility	Responsibility
Responsibility	Interesting work	Interesting work
Security of employment	Security of employment	Security of employment
Hours of work	Tenure of employment	Tenure of employment
Ventilation	Ventilation	Pension fund
Tenure of employment	Lighting	Ventilation
Lavatories and cloakrooms	Hours of work	Lighting
Canteen facilities	Pension fund	Hours of work
Lighting	Lavatories and cloakrooms	Lavatories and cloakrooms
Pension fund	Canteen facilities	Canteen facilities
*Work which requires no thought	Work which requires no thought	Work which requires no thought

WOMEN

Interesting work	Interesting work	Interesting work
Lavatories and cloakrooms	Opportunity for promotion	Opportunity for promotion
Opportunity for promotion	Security of employment	Security of employment
Ventilation	Ventilation	Pension fund
Lighting	Lighting	Tenure of employment
Hours of work	Hours of work	Responsibility
Tenure of employment	Tenure of employment	Lighting
Security of employment	Lavatories and cloakrooms	Hours of work
Canteen facilities	Pension fund	Ventilation
Responsibility	Canteen facilities	Lavatories and cloakrooms
Pension fund	Responsibility	Canteen facilities
*Work which requires no thought	Work which requires no thought	Work which requires no thought

*Unfortunate wording of question probably accounts for constant low ranking.

better able to undertake repetitive work than men and, conversely, the employment of men in women's jobs (as was suggested in the 1930's) is not a corrective for male unemployment.

Validation of standards.—Absence from work due to sickness is a sensitive index of personal efficiency (resistance to disease) and successful adjustment to work and its circumstances. Only 20–40% of absences last more than three days and may be frankly associated with disease. In some groups this percentage is as low as 10. Trivial ailments are a welcome excuse for escape for a few days

from boring jobs and uncongenial colleagues (Fig. 1). Short-term sick absence is an index of emotional or social maladjustment; this accounts for the high rate in adolescents and young adults. The smaller communities in Scotland reduce the ability to be away for trivial reasons. The higher sick absence rate for women may mean that feminine personal efficiency is less, at least so far as acceptance of responsibility is concerned, or that the vicious circle of low responsibility ranking and repetitive work can only be maintained with frequent escapes. Such figures as are available suggest that the employment of young married women does not affect the sick absence rate.
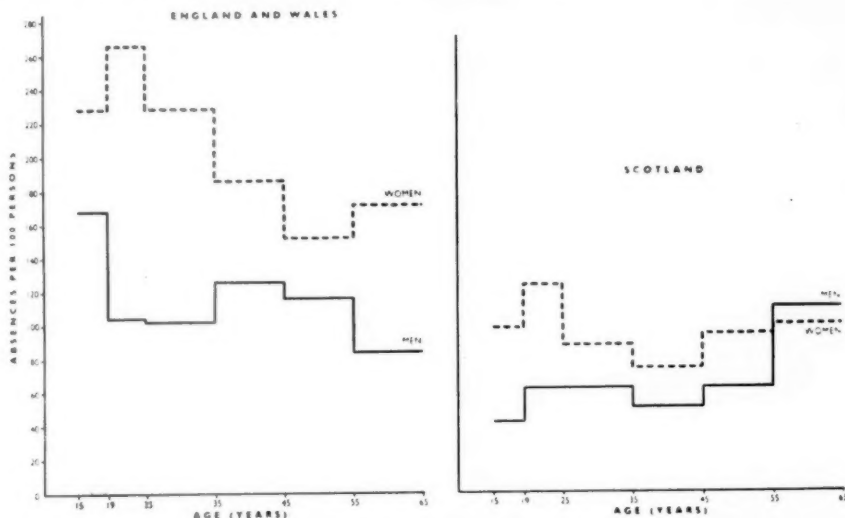
FIG. 1.—Rate per 100 persons in each age group of absences of half a day or more. This pattern is very constant and occurs in different years and in groups with different occupations, e.g. office, factory, shop warehouse workers. The graph shown above is the experience of over 5,000 men and 15,000 women employed in commerce in 1950.

The return rate % at the end of three days in England and Wales is men 60, women 80 and in Scotland, men 60, women 60.

Health.—Health cannot be adequately described in medical, psychological or social terms. To say health is wholeness is to beg the question. Certainly, health requires integration of physical, intellectual and unconscious facets of personality so that the relationship of the whole to society is free from tension. The whole must be free from tension or have the means of tension reduction within itself.

The danger of modern industrial civilization to health is not group and inter-personal stresses resulting from adaptive societies (artificial collections of persons of diverse skills and origins) as described by Elton Mayo (1945) but the blighting of mental and emotional development. Immense and fantastic machines in factories and meaningless symbols in offices create an infantile psychological surrounding (Devereux, 1951). To an adolescent, struggling to replace phantasy with reality, no worse environment could be found. At the best, work is done for incentives which, however laudable, are separate from work.

Can men employed in the place of a machine, as their feed back mechanism is better, who live in identical houses built by society (in the guise of local authorities), who are removed by the iron pipe and delivery van from contact with nature, who expect society to mind, feed and educate their children while their mothers work in artificial societies, who are protected from accident and disease, who give and receive no joy in their work, be healthy? Some force greater than man which impels man to work, so that the opportunity of elemental struggle, poetic truth, artistic intuition or prophetic revelation is in man's work, is necessary to health.

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Major M. M. Lewis (Demonstrator in Army Health, R.A.M. College): *Army health.—Definition*: Health is a biological quality upon which depends man's ability to overcome adverse factors in his environment and to take advantage of those factors which are beneficial.

The Study of Health.

The Army is an ideal community in which to study healthy young adult males; it should be able to make an increasing contribution to our knowledge of this subject.

A considerable amount of work has already been done by Army physiologists regarding such matters as energy expenditure, load carrying and the reactions of normal healthy individuals to extremes of climate. Hitherto, physiological research in the Army has been limited to *ad hoc* studies of specific problems, mostly concerned with the design of clothing and equipment.

In the near future we hope to widen the scope of this research by improving our laboratory facilities, by increasing our establishment of physiologists and by overcoming certain administrative difficulties.

The Army is an ideal community in which to perform health surveys, epidemiological studies and field trials of health-promoting measures. The success of preventive medicine in the Army, particularly in relation to malaria control, water supplies and immunization, is well known.

The fact that the soldier has to serve in a variety of different countries and climates makes his health problems a fascinating subject for study with a wide scope.

The fact that the Army is an easily controlled community makes it relatively easy to study health, to collect statistics and to put health measures into effect.

The Army is one of the few large populations in respect of which reasonably accurate morbidity figures can be calculated without necessarily having to use sampling techniques; accurate figures for the "population at risk" can usually be obtained.

An interesting fact that can be gleaned by studying health in the military community is the size of the contribution to total man-power wastage produced by mental disabilities.

Approximately one-third of all soldiers discharged from the Army on medical grounds are invalided on account of mental disability; the bulk of these are cases of psychoneurosis. This fact reminds us that we must give the study of mental health its proper priority. Mental health in the Army is bound up with a multitude of factors, the most important of which are personnel selection, placement, general administration, man-management and family welfare.

A great deal of pioneer work in relation to medical categorization has been done in the Army. Our Pulheems system, which was derived from a similar system evolved in the Canadian Army, is now used by all three Services. The measurement of health is a difficult matter and I suggest that the Pulheems system has a great deal to offer to those who are doing investigations in this field. The Pulheems system is not ideal. It depends a great deal upon subjective tests, which is a serious difficulty; also, the provision of standards for comparison presents a problem. However, we in the Army find it a workable system, and we are striving to improve it.

The Culture of Health.

The culture of health implies, *pari passu*, the prevention of disease. I will adapt one of the axioms enunciated by the late Sir Thomas Legge to remind us of a basic principle of preventive medicine. Only those measures for disease prevention over which the population has no control are likely to be completely successful; if a measure depends upon action by the individuals who are to be protected it is unlikely to be wholly successful.

For the enhancement of physical health, the military community depends a great deal upon specialists in physical medicine who are particularly concerned with men below standard in physique or with minor remediable disabilities. In the special conditioning courses the following qualities receive attention: agility, mobility, endurance, confidence, posture and strength. The majority of men undergoing these courses are improved mentally as well as physically; in fact, improvement in self-confidence and individual morale is often very striking.

One aim of health education is to teach a healthy way of life, and for this the correct mental attitude to health is important.

Physical function depends a great deal upon motivation, and I suggest that a main difference between an individual who has optimum health and one who has only average health is mental.

We know that physical and mental health are interdependent; the "stress diseases", such as coronary disease and peptic ulcer, have provided a remarkable proof of this.

Perhaps mental health affects physical health to a greater degree than we already realize.

In the Army, all ranks, trades and grades receive health education in some way or other, and every effort is made to ensure that leaders and administrators understand how their actions may affect the physical and mental health of others.

Conclusion.—Finally, I suggest that further work and research regarding health should be pursued along the following lines:

(*a*) The study of the normal, especially the reaction of normal individuals to differing environments, with a view to devising better methods of health assessment.

(*b*) Improvements in health education from the point of view of teaching a healthy mental attitude to life.

(c) Closer liaison between psychiatrists, psychologists and hygienists with a view to tackling the difficult problem of mental health.

Dr. Walter P. Kennedy: The study of health still lacks a basic discipline such as is found in the allied subject of pathology.

I have long been interested in the establishment of "normal" standards in populations, and the variations of these with environmental changes. Recently I have turned more to the fundamental aspects of the problem for I feel that the first essential is to clear up the semantic difficulties. Examination of the literature showed that much loose thinking and no little controversy had arisen from the lack of clear understanding of the exact nature of the subject under discussion.

Many authors have attempted a definition of health, and often admitted that what they could offer was inadequate. Some have stated the word is indefinable. But in logic there are different kinds of definition which can be applied in various circumstances.

Lexical definition is that suitable for a dictionary. It describes, and necessarily inadequately, the connotation of a word in a given culture. Such description is essential for ordinary purposes, but is of little use in serious study. Stipulative definition says in effect, "for the purposes of this present discussion, I stipulate that by health I mean x or y, and I do not imply this will be valid outside the limits of the discussion".

The third and for our purposes the most important type of definition is *real* definition. This is much more difficult to attain for it is concerned with the *res*. No such definition can be short—in fact, real definition would scarcely be recognized as a definition in the ordinary sense of the word. It might even take the form of a book. When one attempts a project such as the real definition of health or some similar term, one often finds that instead of being able to lay down boundaries, the universe of discussion tends to copy the larger universe by expanding continually.

Another aspect of the problem which appears to be of basic importance stems from a technique of modern logic known as the logic of the continuum. Of this I shall mention only the concept of polarity. Health, like light or goodness, cannot be thought of without simultaneously thinking of the opposite, in this case, disease. The law of the continuum states that between the two poles there is an unbroken line of a gradation *uniting* the two. This concept of the essential unity in nature, i.e. kind, between health and disease is consistent with the holistic concept in biology and will be found of the greatest practical use in the establishment of our calculus of health.

Dr. N. H. Mackworth (M.R.C. Applied Psychology Research Unit, Cambridge): It is obviously no easy problem to devise more objective tests of health. Three suggestions can, however, be made for further research. First of all, the greatest need is for a reliable way of producing at will a number of different levels of substandard health in the same people. Different degrees of sleep deprivation might be tried as a source of these quite minor deviations from normal health. Secondly, it would not be enough to test people on only a few occasions. Daily studies of their normal variations in test score should be done initially under normal circumstances. As in the work of Dr. R. B. Cattell, these researches should be intensive daily studies on a few persons week after week and even month after month. Thirdly, the general procedure might consist of measurements based on the idea of discovering how far the men could rise to the occasion. The extra work that men will do when incentives are raised is known to be reduced by the minor ill-health arising from having to work in an overheated room. Perhaps therefore lack of sleep might reduce the improvement in working performance that follows stronger incentives. In fact, the daily performance measure suggested is the difference between activity with and without the incentive of a progress record which gives the subjects full knowledge of their achievement in the current test session.

Dr. Ronald MacKeith: As a teacher of undergraduate medical students I can say that the wide range of normal is now taught. This does not make learning easier for the student and it makes the recognition of the abnormal more difficult for him. Economic factors should be discussed with students from an early stage. Minor maladies are often extremely important and can form valuable teaching subjects in the later part of the clinical period.

Dr. Celia Culver-Evans: I have been deeply impressed to-night by the stress that the previous speakers have laid on spiritual values in relation to good health. And I am convinced that for the improved well-being of the people of this land of ours, closer co-operation between Medicine, Church and the Law is necessary. And especially of the Church. While it is a mother's first duty to teach her child those spiritual values so essential to its well-being, I feel that, in this Christian country the churches—apart from the Roman Catholic which, in this count, definitely has a better record—have been negligent.

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Section of Comparative Medicine

President—A. W. STABLEFORTH, D.Sc., M.R.C.V.S.

[*March* 19, 1952]

DISCUSSION: ACID-FAST BACILLI CULTIVATION
AND DIAGNOSTIC PROCEDURES

Professor R. E. Glover, *Department of Veterinary Pathology, University of Liverpool.*

It is not intended in this short contribution to review the earlier work on methods of isolating acid-fast bacteria, and on the development of various types of solid media. Undoubtedly the most outstanding contributions of recent years arise from the investigations of Dubos and his co-workers on different "wetting" compounds which permit the rapid growth of *Mycobact. tuberculosis* in dispersed form.

Liquid Media

The original Dubos medium (Dubos and Davis, 1946; Dubos, Davis, Middlebrook and Pierce, 1946) has undergone several modifications (Dubos, 1947, Davis and Dubos, 1948; Dubos and Middlebrook, 1948). The most recent formula (Dubos and Davis, 1950), in addition to the salts, asparagine, casein digest and Tween 80, contains traces of Mg, Ca, Zn, and Cu. The albumin fraction is prepared by heating serum at pH 2·0, neutralizing and removing the precipitated globulins. This type of medium has certain drawbacks. Firstly, the Tween 80, which is an ester of oleic acid, is non-ionic and normally non-toxic to acid-fast bacilli, and should be added to the medium at the time of seeding, since there is a danger of its breakdown on storage with the liberation of fatty acids toxic to the organisms (Davis and Dubos, 1948). Secondly, the salt mixture must first be sterilized and is then completed by the addition of separate sterile solutions of Tween 80, yeast extract, glucose and fraction V albumin. Thirdly, the serum fractions which "protect" the mycobacteria against these inhibitory substances may contain lipase which itself may split the Tween 80 (Davis and Dubos, 1948). Although Dubos (1947) has asserted that protein substances other than bovine albumin are unsuitable, it is possible that the globulin fractions of serum might be used. Youmans (1946) and Youmans and Youmans (1950) recently reported that excellent dispersed cultures could be obtained in the medium of Proskauer and Beck to which whole serum or certain globulin fractions had been added. In a recent paper (Glover, 1952) attention is drawn to the value of serum treated with "Liquoid" (sodium polyanetholsulphonate) as a substitute for albumin. Briefly, bovine serum from a non-tuberculous animal is brought to pH 7·4. To each 100 ml. are added 5 ml. of 1 % CaCl$_2$ and 100 mg. of Liquoid. A heavy precipitate containing albumin and a small quantity of globulin is removed. The supernatant

portion which is sterilized by filtration is added to a slightly modified Dubos medium in which the vegex is replaced by yeastrel (Paterson and Winmill, 1950) in the proportion of 0·2 ml. serum to 5 ml. of medium. The growth of human, bovine and avian strains is more rapid than in the conventional medium of Dubos.

It would be of advantage to incorporate substances of simpler composition than asparagine or casein digest as a source of nitrogen. Dubos (1947) has shown that in the presence of Tween 80 dispersed growths can be obtained in a medium in which ammonium sulphate is incorporated as the main source of nitrogen. Ackart (1951) has recently reported that *Myco. tuberculosis var. avium* will propagate very rapidly in a Dubos type of medium containing succinic acid in the place of glucose as the main source of carbon, and ammonium succinate as a source of nitrogen. This observation has been confirmed and an excellent medium has been devised as follows: disodium hydrogen phosphate, 6·3 grammes; potassium dihydrogen phosphate, 1·0 gramme; sodium citrate, 1·5 grammes; magnesium sulphate, 0·6 gramme; ammonium succinate, 0·8 gramme; succinic acid, 1·2 grammes; iron ammonium citrate, 0·02 gramme; glycerin, 2·0 ml.; water to 1,000 ml. It will be noted that yeastrel, asparagine and glucose have been omitted and that there is no Tween 80. It has recently been found that the latter can be added to the Liquoid-treated serum (25 ml. of a 10% solution to each 100 ml. of treated serum) before filtration. This serum mixture can be stored at 0° C. indefinitely without any evidence of break-down of the wetting agent, and is added to the salt mixture (0·2 ml. to each 5 ml.).

In simplifying media by the omission of complex substances such as casein digest and yeast extract, care must be exercised lest factors essential for growth are inadvertently omitted. Baisden (1951) has observed that certain samples of asparagine obtained in 1950 were unsuitable for the growth of *Myco. tuberculosis* in the Bureau of Animal Industry medium of Dorset currently used for tuberculin production, whereas later samples were quite satisfactory. A chemical analysis showed that recent samples were more highly purified and were lacking in Zn, which, as demonstrated earlier by Henley (1940), is necessary for the metabolism of acid-fast bacteria. A similar phenomenon was encountered in our work with a sample of iron ammonium citrate in which a batch of recent manufacture gave very poor results, growth being much less active than in Dubos media. An old batch, however, was satisfactory. It was found that the latter contained appreciable amounts of both Zn and Cu which were absent from the newer sample.

The observation of Davis and Dubos (1948) and Taylor (1950) that the addition of the growth-promoting factors of *Myco. phlei* to Dubos medium fails to render it suitable for Johne's bacillus has been confirmed. However, the asparagine medium with Liquoid-treated serum has proved satisfactory provided it contains about 3% glycerin extract of *Myco. phlei* prepared by the method of Taylor (1950). At first strains grew very slowly and sparsely, and visible growth was not observed before the third week, but on repeated subculture the rate of growth was increased and the period of incubation was shortened. Two strains were well established after five passages, three were more difficult to adapt, while three were completely refractory.

Allen (1952) has reported that in a simple asparagine medium without phlei extract a marked growth of Johne's bacillus was observed after six weeks, but he does not state whether the organisms were in a dispersed state. Very rapid growths have now been secured in the ammonium succinate, succinic acid medium with phlei extract: moreover, ten strains have been established including the three just mentioned which failed to grow in the earlier type of medium. So far it has not been possible to isolate Johne's bacillus from natural material. The medium has also been most useful for obtaining dispersed growths of the vole bacillus.

Solid Media

Treatment of contaminated material containing acid-fast organisms.—The choice of an agent for the elimination of contaminating organisms is still a matter of individual preference. The original method of treatment with NaOH or antiformin has been supplemented by sulphuric acid, hydrochloric acid, oxalic acid, and, quite recently, trisodium phosphate. The problem is to select an agent at a suitable strength and allow it to act for a suitable period so that all the contaminating organisms are destroyed without undue harm to the acid-fast bacilli. This ideal has certainly not yet been attained: moreover, the problem is particularly complex in the veterinary field where it is frequently desirable to isolate acid-fast bacilli from faecal material heavily contaminated with spore-bearing organisms.

In many cases claims have been made that a new agent is more satisfactory than one in current use because when tested on tuberculous sputa a higher proportion of positive results, usually based on a cultural examination, is obtained. The experiments often fail, however, to give any information on the proportion of acid-fast bacilli which is still alive after treatment. The following experiment shows the effect of NaOH. A sample of normal sputum was emulsified with a saline suspension of *Myco. tuberculosis*. The infected sputum was then treated with 4% NaOH at 37° C. for varying periods. Serial dilutions of the original material and of the NaOH-treated samples were then inoculated into guinea-pigs with the results shown in Table I.

TABLE I

Dose M. tuberc. bovis	C (A)	C (B)	10	20	30	40	60
1 × 10⁻⁸	—	—	—				
	—	—	—				
	—	—	—				
1 × 10⁻⁷	—	—	—	—	—		
	1	1	—	—	—		
	1	2	1	—	—		
1 × 10⁻⁶	3	3	1	—	—	—	
	3	2	2	1	1	—	
	1	2	3	1	1	—	
1 × 10⁻⁵	3	3	3	1	1	—	—
	3	3	3	1	1	1	—
	3	3	3	2	2	2	—
1 × 10⁻⁴				3	2	2	2
				3	3	3	2
				3	3	3	2
1 × 10⁻³					3	3	2
					3	3	2
					3	2	1

C(A) = unincubated   Each symbol = 1 guinea-pig   — = no lesions
C(B) = incubated   1 = slight lesions
                                                  2 = moderate lesions
                                                  3 = generalized lesions

In my hands treatment of heavily contaminated material with alkalis has given the most consistent results. Oxalic acid is certainly useful, but it is doubtful if the recently introduced sodium phosphate is entirely satisfactory. Corper and Stoner (1946) claimed better results than with any other agent. Gifford, McKinley and Hunter (1951) also obtained superior results with phosphate as compared with NaOH. Beattie (1949), on the other hand, states that in heavily infected sputa, phosphate affected the acid-fast organisms adversely as compared with hydrochloric acid.

The incorporation of dyes such as gentian violet, malachite green, &c., in the medium with the object of restraining the growth of non-acid-fast organisms is of doubtful value. If preliminary treatment fails to suppress the contaminants the dyes are unlikely to do so: moreover, some of them appear to delay the growth of the acid-fast bacilli.

Value of Egg Media

Earlier workers, particularly A. S. Griffith, were most insistent on the value of Dorset egg media, with and without glycerin, for the primary isolation of tubercle bacilli. Unsatisfactory results which have been reported may have been due to the liberation of certain fatty acids from egg yolk which have a delaying effect on growth. For example, Corper and Clark (1946) showed that toxic fatty acids may be set free in stored eggs. These inhibitors which are soluble in acetone (Eggerth, 1950) are highly bacteriostatic (Buu-Hoi, 1945). The most active are those containing 12 to 16 C atoms, e.g. palmitic (Drea, 1944).

Efforts have been made to improve egg medium by the incorportion of asparagine, liver extract, casein, potato extract or starch. Some doubt may be expressed as to whether the advantages claimed for the complex media such as Löwenstein, Petragnani, and so on, have been as significant as has been alleged. In some cases the addition of various substances to egg media may have had an unfavourable effect. For example, it has been shown that the incorporation of glycerin potato extract to an egg-yolk medium, officially recommended by the Trudeau Society in 1946, is not only unnecessary, but may result in a delay in the appearance of colonies. In designing new media it is important to avoid any process which may permit the liberation of toxic fatty acids. It is of interest that there is a tendency to return to the simple egg media. Attention may be drawn to the observation of Taylor (1950), that a medium with a high yolk content has given a much higher percentage of positive cultures of Johne's bacillus than the more complex media formerly used.

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Dr. S. D. Elek: *The Serodiagnosis of Tuberculosis.*

There is a great need for a reliable serum diagnostic test for tuberculosis. Sera can be examined in large batches and the methods are generally economical in labour and materials. In the human field it would be a useful adjunct to mass radiography as it would be equally effective in extrapulmonary forms of tuberculosis. In the veterinary field it might clarify the information obtained from the tuberculin test. Not unnaturally, therefore, attempts were made at a very early date to develop such a test and in 1898 Arloing and Courmont advocated an agglutination test using a special strain of tubercle bacilli. This test proved to be totally unreliable for diagnostic purposes. Next came the application of the complement-fixation reaction, which gained fairly widespread acceptance in France, but was never popular in this country. The figures quoted by Rieux (1926) appeared hopeful enough but the number of false positives was great. The test is generally positive in chronic pulmonary tuberculosis, but such cases can be readily diagnosed by other means, and in early disease the results of the complement-fixation reaction are unreliable. There was little interest in this field until recently when Middlebrook and Dubos in 1948 described an ingenious technique for the demonstration of antibodies to tubercle bacilli. The technique consists of coating red cells with a bacterial product, and cells so treated become agglutinable by antibodies acting against it. The agglutination of red cells is easy to observe and the test has become known as the "hæmagglutination reaction in tuberculosis". Middlebrook and Dubos showed that, by its means, antibodies can be demonstrated in the sera of experimentally infected tuberculous animals. Beyond that they did not go. Rothbard, Dooneief and Hite (1950), however, reported very favourably on the test when they tried it on human cases in New York and similar reports came from Gernez-Rieux and his co-workers (1949) in France. For our work we have chosen a somewhat simplified technique (Hilson and Elek, 1951). In the original method sheep red cells were used, but since human sera contain agglutinins to them, a preliminary absorption had to be carried out. We used human group O cells and a capillary agglutination test of proven value in Rhesus work. Apart from these technical modifications the test was the same—but our results did not agree with the findings of the earlier workers (Table I).

TABLE I

Authors				Tuberculous sera Total	% positive	Control sera Total	% positive
Gernez-Rieux and Taquet, 1949	..	..	..	95	80	43	Nil
Rothbard, Dooneief and Hite, 1950	..	..	168	92	216	Nil
Sievers, Ulstrup and Winblad, 1950	..	..	?	53	102	17
Smith and Scott, 1950 (titres of 1:2 and over)	..	104	80	81	50
Sohier, Juillard and Trimberger, 1950	..	..	30	66	130	15
Hilson and Elek, 1951	..	..	..	66	22	60	Nil
Kirby, Burnell and O'Leary, 1951	..	..	..	54	57	251	10

Those workers who obtained a high percentage of positives in tuberculous patients also found a high percentage of false positives. The relationship of the percentage of correct positives to the percentage of false positives constitutes the index of the reliability of a test of this kind. Ideally, of course, all the genuine positives should be recovered while there should be no false positives. To a limited extent this problem is technical. Perhaps it is not sufficiently realized, that by adjusting the sensitivity of a serological test it is possible to abolish all the false positives, but generally there is also a reduction in the percentage of genuine positives. This is well illustrated by the behaviour of the Wassermann and precipitation tests used for syphilis.

The hæmagglutination test was originally carried out with Old Tuberculin. This showed a test of poor sensitivity and rather large numbers of false positives. Somewhat better results were obtained when a carbohydrate extract was substituted for crude tuberculin. Another observation made was that heating of human sera to 56° C. for half an hour increased the specificity of the test. Furthermore the keeping of fresh sera in the unheated state even at 5° C. resulted in a considerable loss of the antibody. A somewhat similar paradoxical state of affairs exists with human sera used for Rhesus work: there is a loss on keeping although the antibody is fairly heat stable. It suggests that proteolytic enzymes present in the serum destroy the antibody, while heating of the sera will inhibit this mechanism. It would be particularly interesting to know whether this anomalous behaviour is a property of human sera, or whether a counterpart exists in the veterinary field.

With all the modifications, however, we were unable to improve the test beyond giving one-quarter of the positives in the tuberculous group *at a level at which no false results were encountered.*

It might be useful to compare the requirements in human pathology and contrast them with those in the veterinary field. In medicine we are not so much concerned with the diagnosis of tuberculous infection which is accessible by other means, but we would like to assess the activity of the disease, that is to say the immune state of the individual patient. In the veterinary field the problem is essentially different: it is the earliest manifestation of infection that is sought with a view to the complete eradication of disease from domestic stocks. While in animals we are dealing with virgin soil so far as tuberculosis is concerned, in medicine at present we have to assume that everybody is infected or potentially exposed to infection, and what we are primarily interested in is the measurement of an individual's capacity to resist the infection. If we employ a serological approach, the essential question is whether the antibodies detected by these means reflect the immune state of the animal. The production of antibodies is a general reaction of the body: the protective action of certain antibodies is probably only incidental. In the course of an infection antibodies are formed against a very large number of bacterial constituents. Some of these bacterial antigens have no role in the development of disease and the antibodies formed against them will be of no particular use to the patient. Yet other bacterial antigens are essential for the pathogenicity of a given microbe for a particular host and antibodies formed against them will have a protective role. As we know more and more about the antigens which are essential for the pathogenicity of a given microbe for a given animal we tend to link the antibodies formed against them with the diagnosis of a particular disease. For instance, in the Widal reaction, the Vi antibody is obviously of greater significance than the flagellar antibodies. One might almost say that there are two kinds of bacterial antibodies: those that throw some light on the immune state and those which are accidentally present. While both kinds could be used for diagnostic purposes, the former are obviously more important. If we follow up the analogy of the Widal reaction, the problem of a tuberculous patient is analogous to the interpretation of the Widal reaction in an inoculated individual. Should such an individual develop typhoid, a reliable serological guide would be the demonstration of an increase in true protective antibodies of the Vi type.

Does the hæmagglutination reaction reveal the presence of such a protective antibody? The fact that only a proportion of tuberculous individuals give the reaction in a significant titre suggested to some of the workers that it might reveal clinical activity. Claims in this respect have been made by Rothbard and his co-workers and by Smith and Scott. At present we are without a single reliable criterion of activity and it has to be assessed on a combination of observations. Recent work suggests that the claims made cannot be substantiated. There does not appear to be any correlation between positive tests in tuberculous patients and the clinical assessment of the case. Likewise the hæmagglutination reaction titre is not found to be correlated with erythrocyte sedimentation rate, duration of disease or the presence or absence of a positive sputum.

It is possible, however, that the hæmagglutination test may find a limited usefulness in the veterinary field. Avian tuberculosis presents no problem in human pathology, but in the bovine species hypersensitivity reactions to tuberculin have to be differentiated from infections with the mammalian types. The allergic reaction is based on protein fractions which are fairly widely distributed amongst acid-fast bacilli. At present only quantitative differences can be used for the differentiation of avian and mammalian allergic reactions. It is quite clear, however, that the protein derivatives play no part in the hæmagglutination reaction. The coating agent is almost certainly of a polysaccharide nature and can be obtained in a concentrated form after removal of the proteins by trichloracetic acid precipitation.

There is reason to think that the tuberculin reaction overestimates the number of mammalian infections in cattle. Our investigation of a single strain of avian tubercle bacillus indicated that the polysaccharide fraction may not be common to both avian and mammalian strains. It is possible

therefore that the hæmagglutination reaction will be of some use in the veterinary field especially since in this virgin soil a low titre would be significant.

To sum up, the hæmagglutination reaction is of very little practical use in medicine in spite of the enthusiasm with which it was received. If circulating protective antibodies play a part in the natural history of tuberculous infections they have yet to be found. None of the fractions of tubercle bacilli so far investigated can serve as the basis of a satisfactory serological test and we have no knowledge on what chemical fractions the virulence of the tubercle bacillus rests. Once this gap is safely bridged we can look forward to a serological test giving an indication of the immune state of an individual—if indeed the immune state is associated with circulating protective antibodies.

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Mr. P. Stuart: *Bovine Mastitis Resembling Tuberculosis Caused by Rapidly Growing Acid-fast Bacteria.*

Ten cases have been diagnosed up to the present, all of which had very similar histories. The cows were at first affected with a more or less acute mastitis which was probably caused by streptococci. All were treated by intramammary infusions of penicillin-oil-wax suspension or of diamino-diphenyl sulphone in oil, or of both of these, and the acute reactions subsided. This was soon followed by chronic changes which resulted in enlarged, painless, indurated quarters suggesting tuberculosis of the udder, and acid-fast bacilli were found in films of milk at this stage. One of the first cases diagnosed has been under observation for more than a year. The affected quarter still remains enlarged and indurated, and the acid-fast bacilli are still being excreted.

In films of milk deposit from affected quarters the bacilli tended to be a little stouter and more pleomorphic than *Mycobacterium tuberculosis* and varied more in brightness of red after Ziehl-Neelsen staining. Groups of endothelial cells which are an important diagnostic feature of tuberculous milks were present in some of the infections but in these cases the bacilli were not concentrated in or around the cell groups.

The cut surface of affected quarters had a mottled appearance similar to that of diffuse granulomatous tuberculosis but no lesions were present in the supramammary lymph nodes. The lesions in the nodes were granulomata which contained some giant cells, but in the centre vacuoles were present which contained a globule of oil or fat. The acid-fast bacilli were concentrated in these globules and only a few were seen in the surrounding cellular zone.

Cultural tests showed conclusive differences from cases of tuberculosis and confusion would arise only in mixed infections. Colonies of the organisms were visible in two days and they grew as well on blood agar and glycerin agar as they did on egg media. The colonies were at first moist, smooth and greyish but later they became drier and pigmented to a yellow or almost orange colour. The organisms from young cultures were bacillary and most were acid-fast, although there was always a small proportion which were non-acid-fast; in older cultures they became cocco-bacillary and the proportion of non-acid-fast organisms increased with the age of the culture. When cultures were prepared from affected udder tissue the organisms withstood treatment with 5% oxalic acid for thirty minutes or more, but those in milk deposit were killed by such treatment, and were greatly reduced in numbers in as short a time as five minutes. Blood agar containing 1 in 30,000 malachite green is now used as a selective medium for primary cultures without prior oxalic acid treatment of the materials.

The methods of Merril (1931), Gordon (1937), Gordon and Hagan (1938) and Thomson (1932) were used in identifying the bacilli. These are based on temperature range of growth, heat survival, carbohydrate utilization and colonial appearance. The strains isolated from 8 cases belonged to Thomson's Group I and followed the description of *Myco. lacticola* given in the 6th edition of Bergey's Manual. Another similar organism isolated from one case fell into Group II of Thomson's classification whilst a third did not fit into any group, and, although it was strongly acid-fast, it may not belong to the Mycobacteria. A description of these organisms has been given in a previous publication, Stuart and Harvey (1951).

On account of the similarity of these cases to tuberculosis a considerable number of biological tests were carried out on milk samples and affected tissues in guinea-pigs, rabbits and chickens. Macroscopic lesions were produced only when milk deposit plus cream was inoculated into guinea-pigs subcutaneously; the lesion was a local abscess which contained acid-fast bacilli. When pus from these abscesses was inoculated into further guinea-pigs no lesions resulted. Similarly cultures in saline suspension caused no lesions, but oily suspensions of cultures caused multiple abscesses in the lungs and kidneys when injected intravenously into rabbits.

The affected cows gave no reaction to either avian or mammalian tuberculins injected intradermally and guinea-pigs inoculated with infected materials or cultures also gave little or no reaction to these tests. A P.P.D. was prepared from the Group I and from the Group II Mycobacteria by A. B. Paterson at Weybridge Laboratory. Four heifers which had been inoculated intradermally with oily suspensions of these organisms reacted specifically to the corresponding P.P.D. and not to avian or mammalian tuberculins, and the one field case of Group I Mycobacterium infection which was tested with these preparations, reacted only to the Group I Mycobacterium P.P.D.

When suspensions of cultures of the acid-fast bacilli were infused into normal lactating and non-lactating bovine udders no clinical change occurred and no abnormality could be detected in the tissue microscopically when the cows were slaughtered four to six months later. The infusion of normal lactating udders with oily suspensions of cultures resulted in a mastitis indistinguishable from the field cases. This also occurred when tubes of penicillin-oil-wax suspension were contaminated with cultures of the bacilli and infused into the udder. The acid-fast bacilli were recovered from milk samples in large numbers until the animals were slaughtered. The macroscopic appearance of the cut udder tissue, the cultural and histological findings in these experimentally produced cases were the same as in the field cases, and the oil globules containing masses of the bacilli were present.

The udder of a control cow was infused with sterile tubes of penicillin-oil-wax suspension and with sulphone in oil. No abnormality occurred and no lesions could be found in sections of the infused quarters.

It would seem, from the findings in the field cases and from the experimental infections of udders, that both oil and the acid-fast bacilli are essential for the production of lesions. The pathogenic effect of the rapidly growing acid-fast bacilli in oil has been recognized in experimental work in laboratory animals since Rabinowitsch demonstrated it in 1897 using butter containing the butter bacillus, but the only importance generally attached to the presence of these organisms in diseases of man and domestic animals is on account of their morphological similarity to, and possible confusion with *Myco. tuberculosis.* Beaver and Bayne-Jones (1931) describe a case of pulmonary disease resembling tuberculosis in a child, and which was apparently caused by an organism not unlike those we isolated from the bovine udders.

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Mr. N. H. Hole: *Complement-fixation Tests and the Mycobacteria Infections.*

My contribution is based mainly on some experiments we are carrying out at Weybridge to determine the practical value of complement-fixation tests in the diagnosis of Johne's bacillus infection in cattle. These experiments are in progress, and this communication is one of preliminary rather than of conclusive findings.

This serological method of diagnosis has been tried in the past, and has been discarded mainly on the grounds of non-specificity. We have no reason to suggest that the methods we are using are any more specific than those used by other workers. We know that a cross reaction is obtained with bovine tuberculosis, and probably with avian tuberculosis, but if we exclude these two conditions we are not convinced that non-specificity is a factor of major importance. Before assessing the value of our early results, it is necessary briefly to consider the meaning of infection in Johne's disease. The clinical condition is well known, but it is now quite certain that in this disease, as in tuberculosis, infection may be present in apparently normal animals. Our evidence is that extensive lesions may be present without symptoms, and that it is by no means certain that infection will ultimately terminate in the clinical disease. We have, therefore, a problem within a problem; we consider that our preliminary results suggest that the methods we are using are a promising means of determining infection during life, but we have no reason to suppose that these tests will prove to be of value for deciding which reacting animals are liable to become clinical cases. On the other hand, as soon as an animal is clinically suspect, the test has proved a very accurate means of determining whether the symptoms are due to Johne's disease. In this respect, in our experience, it has proved far superior to other, non-serological methods of *in vivo* diagnosis.

The test we are using is a modified simple hæmolytic complement-fixation test, in which guinea-pig complement, sheep red cells and anti-sheep horse hæmolysin are used. We have tried several types of antigen, including modifications of Nègre's alcoholic extracts of the organism and the tissue extracts of Sigurdsson, but for routine use we have reverted to the simplest antigen, namely a saline suspension of washed Johne's bacilli. It is the use of such a particulate antigen that enables us to apply a modification in technique which we have found most useful, in that it avoids the anti- or pro-complementary action of the test sera. A wide range of such complement interference is met with in

testing large numbers of individual bovine sera. It is quite a common practice to reduce such anti-complementary action by raising the temperature of inactivation. With the mycobacteria we consider this may be dangerous, because we have a strong suspicion that the antibody, or part of it, is not very thermostable. We therefore inactivate our sera for half an hour at 56° C. Dilutions of the sera are then mixed with the standard dose of antigen, and are left in contact for one hour at room temperature to allow any antibody present to become adsorbed by the antigen. The organisms are then spun down, the supernatant is discarded, and the deposit is resuspended in fresh saline. The dose of complement is added to this serum-free suspension, and half an hour at room temperature is allowed for fixation to occur if the antigen has been sensitized. The hæmolytic system is then added and the whole is incubated in the usual way. The dose of complement is estimated in the presence of the antigen. This modification adds to the labour of the test, but we consider the standard results obtained make it worth while.

Complete fixation with a serum dilution of 1 in 5 is regarded as positive. The serum of a very strong reactor may have a titre of 1 in 80. The titre does not appear to be related to the extent of the lesions.

The specificity of the reaction is obviously dependent on the selectivity of the antigen. We have not found any of the antigens we have used to be capable of directly differentiating antibodies to bovine or avian tubercle bacilli from antibodies to Johne's bacillus. Experience suggests that we are detecting a group antibody, and that the antigen involved is predominantly present in the bacillary antigens and alcoholic extracts of the avian and Johne's bacilli; it is less readily obtained from bovine and human strains. Curiously, in a few experiments in which we tried P.P.D. tuberculin as an antigen, the reverse proved to be the case: the mammalian preparation was found to have quite a good anti-genic titre, but the avian preparation was of very little practical value. The obvious inference is that a different antigen-antibody complex is involved. The few tests done with P.P.D. antigen did not suggest any greater specificity, and corresponding results were obtained with the organismal antigen. This is an aspect of the work to which we hope to give more attention.

We have applied this test to naturally occurring Johne's disease in the field, with two main objectives. The first has been to determine its possibilities as a means of confirming the existence of the disease in clinically suspected animals. Preliminary results have been very satisfactory, for in about 95% of such cases that we have been able to follow to slaughter, the test result and the post-mortem findings have agreed. At present we have complete data only on about 60 such animals, mostly positive cases; we hope to make our final assessment on a much greater number, with a more even distribution of positives and negatives. The second objective is, by regular blood testing, to build up a serological picture of all the animals in a number of infected and a few non-infected herds, with the ultimate object of following animals from birth to death, and of correlating the blood reactions in life with the post-mortem findings. We have avoided the complication of bovine tuberculosis by using only attested herds; true avian tuberculosis in the bovine we believe to be comparatively uncommon. It is too early in this experiment to assess the results; in some severely affected herds the test has proved a remarkably successful means of forecasting future clinical cases, but in other herds, although the results suggest it may prove a reasonable method of detecting infection, the liability of reacting animals to become clinical is not nearly so marked.

I will conclude with two further observations. Firstly, a serological response appears to develop very slowly after infection; in the experimental disease it may take as long as nine months. Secondly, our earlier statement that there is a cross-reaction with bovine, and probably avian tuberculosis, needs amplification. In the few instances in which positive mammalian tuberculin reactors have been tested by the Johne's complement-fixation test, the result has always been positive; on the other hand a positive reaction to avian tuberculin does not appear to be connected with the serological response. We are differentiating between infection and reaction to the tuberculin test. The interpretation of the official tuberculin test in the bovine depends largely on the comparative response of an animal to mammalian and avian tuberculins; an animal developing a mammalian reaction in excess of the avian is considered to be infected with bovine tuberculosis, and this has proved to be a very accurate means of diagnosis. Where the response to the avian tuberculin is the greater, the reaction is ascribed to avian or non-specific infection. Time does not permit us to enlarge on the interesting comparison of tuberculin and complement-fixation tests. We simply state that animals classed as bovine tuberculin reactors have invariably reacted to our Johne's serological test; the reverse does not apply. Avian tuberculin reactors, on the other hand, may be complement-fixation positive or negative. Many serologically positive and Johne's infected animals are completely negative in their reaction to either tuberculin. Our present opinions on this point may be summarized by stating that we believe our complement-fixation method may prove group specific for bovine and avian tuberculosis and Johne's disease, and thus exclude a number of non-specific conditions that apparently may give rise to avian tuberculin reactions. It is true that certain cases of so-called skin tuberculosis have reacted serologically; many do not. We have had no opportunity yet to prove, in the reacting cases, that there is not a concurrent infection with Johne's bacillus. This possible greater selectivity of the complement-fixation test is a significant consideration that needs much confirmatory evidence before it can reasonably be accepted.

CONTENTS (*continued*)

CONTENTS (*continued*)

N.B.—The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

Section of Urology

President—Arthur Jacobs, F.R.F.P.S.

[*January* 24, 1952]

The Classification of Male Hypogonadism [*Abstract*]

By William S. Mack, M.B., Ch.B., F.R.C.S.Ed.
Royal Infirmary, Glasgow

It is now becoming possible to make a tentative classification of the types of male hypogonadism thus enabling treatment to be placed on a rational basis. R. Palmer Howard *et al.* (1950) and Heller and Nelson (1948) have published very useful articles on this subject.

Testicular failure may be primary in the gland itself or it may occur secondarily from inadequate function of the anterior pituitary. In classification, patients can be separated into four groups due to

 (1) Failure of the whole anterior pituitary function.
 (2) Failure of gonadotrophic function of the pituitary.
 (3) Leydig cell failure.
 (4) Seminal tubule failure.

The clinical picture is profoundly modified by the time of onset of the failure, i.e. whether or not it occurs in childhood or after full adult development has been reached.

It is not possible to classify male hypogonadism by the clinical appearances alone and other tests must be employed.

 (1) Estimation of gonadotrophic excretion in the urine is probably the most valuable of all since a lowered value indicates pituitary failure and a raised value is found in primary testicular failure.

 (2) If it is impracticable to carry out a gonadotrophic excretion estimation the patient's response to pregnancy urine extracts can be tried as a means of differentiation; failure to respond means that the fault lies with the testis itself.

 (3) Androgens are produced by the Leydig cells and also by the adrenal cortex. The amount present in the urine can be estimated biologically but it is easier to estimate, chemically, the amount of 17-ketosteroids excreted during the course of twenty-four hours and to use this figure as an indirect measure of androgenic activity. A lowered value will be found in Leydig cell failure.

 (4) Testicular biopsy shows the actual histology of the testis but interpretation is difficult and perhaps misleading as when a great increase in the numbers of Leydig cells is found to be associated with definite evidence of Leydig cell hypofunction. In spite of its limitations biopsy ought to be carried out oftener than it has been since it may give the clue to the essential cause of the condition present. Biopsy is also of great value in the investigation of cases of infertility since these patients frequently show no evidence of any endocrine upset though various syndromes have been described by Castillo (1947) and by Klinefelter (1942) where azoospermia was found in association with hormonal alterations.

 (Slides illustrating the diagnosis and treatment of eunuchoidism and other types of male hypogonadism were shown.)

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Replacement of the Ureter by Small Intestine. An Experimental Study
[*Abstract*]

By David Annis, F.R.C.S.
Lecturer in Surgery, The University of Liverpool

When wide excision of a part of the ureter has left a gap that cannot be repaired by direct anastomosis it may be necessary to remove a useful kidney.

As an alternative to this we tested the feasibility of using an isolated length of small intestine to replace the ureter.

In two dogs between a half and three-quarters of the left ureter was removed and replaced by an isolated length of small intestine with its blood supply intact. To place the whole burden of excretion upon this left kidney with its intestinal ureter, the right kidney was removed at a subsequent operation (Fig. 1). The dogs survived and could not be distinguished from normal animals. They remained healthy until they were killed about twelve months later.

The removal of this right kidney resulted in a rise in the blood non-protein-nitrogen levels from about 60 mg.% to a level of 140 mg.% in one dog, and 100 mg.% in the other. These gradually
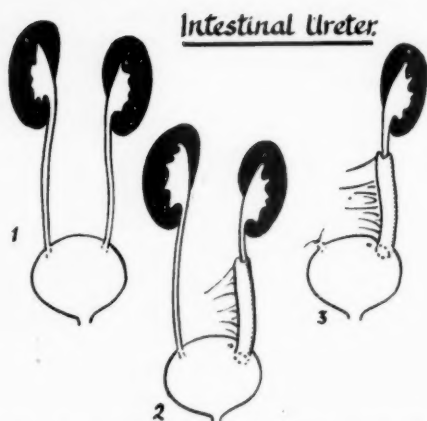
Intestinal Ureter

Fig. 1.—1.–Normal kidneys, ureters and bladder. 2.–Replacement of the left ureter by an isolated segment of small intestine. 3.–Subsequent removal of the right kidney.

fell during the subsequent weeks. Final levels of 69 mg.% and 88 mg.% are higher than the upper limit of normal values for the dog. After about 12 months the dogs were killed. At post-mortem, grossly, in both dogs the kidney appeared normal and the renal pelves were not dilated in either dog. The stump of the ureter below the kidney was of normal diameter in one dog and showed a very slight dilatation in the other. The segment of small intestine seemed unchanged and was not dilated. Microscopic section of the anastomoses showed a direct continuity of epithelium between the ureter and the intestinal loop at the upper end and the intestinal loop and the bladder at the lower end. These experiments show that life was satisfactorily maintained for the duration of the study, on a single kidney connected to the bladder by an intestinal ureter.

Is there a practical application of this method in the surgery of human disease?

In the past there have been many attempts to find something that could be used to replace the diseased or damaged ureter. The use of small intestine was considered many years ago.

Somewhat similar operations in man have been described by Shoemaker, by Longuet (1948), and by Muller.

These clinical reports and the experimental evidence suggest that it might be possible to replace the diseased ureter by a length of small intestine. It might be found useful where the excision of a carcinoma of the colon requires the removal of a length of invaded ureter. It might be used in the treatment of patients after nephrectomy for tuberculosis, when multiple ureteric strictures are producing hydronephrosis and destruction of a sole remaining kidney.

REFERENCE—LONGUET, Y. J. (1948) *Urol. cutan. Rev.*, **52**, 322.

Treatment of Vesical Neoplasms by Closed Radium Applications [*Abridged*]
By A. R. C. HIGHAM, F.R.C.S.

THE current methods of treatment of bladder growths with radium are not at all satisfactory, mainly, I think, because we have to open the bladder to insert the radium, and then cannot sew it up again at once because we need to retrieve the needles after a week or so. This leads to two grave disadvantages, one due to difficulties of spacing the radium, the other to a peculiarity of healing in the presence of irradiation.

With the bladder open, it is not difficult to insert needles in roughly the desired position in or around the tumour. The drawbacks become obvious as soon as the suprapubic tube has been inserted and the bladder sutured around it. From being a plane surface the walls contract into a sheath for the tube, leading from the trigone to the belly wall, and the radium needles, once so neatly spaced at even distances approved by the physicist, become jumbled together.

Further, the effect of radiations on the suprapubic drainage wound is to delay the healing of the fistula, so that the admission of a patient for a period of irradiation lasting six days results too often in his being kept in for six weeks while a sinus heals.

These disadvantages can be obviated by arranging the needles, before insertion, on a flexible base which is applied to the area to be irradiated and which can be extracted through a perineal urethrostomy without the necessity for suprapubic drainage of the bladder.

The needles can be fixed so that they remain in the prearranged pattern, and exert an even, controllable effect on the subjacent tissues. The physicist can be sure that his isodose curves really represent similar effects on the tumour cells without unexpected local areas of overdose or of weakness of field.

And changes in volume of the tissue being irradiated do not alter the relative positions of the needles, nor the strength and evenness of the field irradiated. This latter point can become important when a large area of bladder wall can be made to contract and so bring the possibly malignant cells in its thickness into the effective zone of irradiation. This principle, of making the bladder contract to a small volume, less than, say 50 millilitres, has a disadvantage in that the larger the diameter of a sphere, the more nearly does its surface approximate to a plane, and it is as a plane implant that the usual isodose curves are calculated by the physicist. But corrections can be applied for small and predictable variations from plane implants.

Finally, if an incision through the bladder wall does form a necessary part of the treatment, it can be

allowed to heal by first intention before the evil effects of radiation have been exerted, and a thin linear scar will result, around which deep X-rays can be used if desired, or further surgery can be employed.

The details of the method of application depend on the thickness of the tissue affected by neoplastic cells. Isodose curves demonstrate that for needles of 1 mg. per cm. the intensity of the radiation at a depth of 25 mm. from the surface of the plane of implant is about 5 r per hour. Even using 5 mg. per cm. as is now done, to produce 5,000 r at a depth of an inch we need a stay of 8·35 days, so that it is desirable to deal with thinner slices if it is at all possible.

The most radical method is to open the bladder and to scrape out the growth with a large diathermy loop. This is done until it appears that the whole of the indurated area has been removed. Now another thin sliver of tissue from the whole base is taken and is placed in "Bottle B"; this will provide histological evidence of spread if present beyond the line of excision.

The area of the resulting ulcer is measured, and a stencil of suitable size is selected. Radium needles are placed through the slots in the stencil and adhere to the sticky side of a rubber patch; another patch is applied over the needles and a stay suture is passed through one corner of the patch. Through a perineal urethrostomy a pair of forceps is passed into the bladder and the suture is pulled out and anchored. A Foley catheter with a bag of 50–100 c.c. is inserted into the bladder through the perineum, and the patch is carefully applied to the ulcer; if it is small, a stitch is passed across the front to fix it. Larger patches do not need this. The bladder is sewn up without drainage and the catheter is distended.

At one time it was thought advisable to shield the remainder of the bladder wall by putting some radio-opaque fluid into the bag, but experiments with a tetrahedral bag filled with metallic mercury, as the most opaque fluid known, have shown that the shielding effect is not much more than that caused by any fluid holding the wall away from the patch; and as a small amount of radiation may well be beneficial in discouraging secondaries in the suture line or in other parts of the bladder, plain coloured water is now used:

The aim, now, is to remove at least the main bulk of the tumour and to leave a manageable thickness of bladder wall behind, which can be irradiated by a single layer of radium needles. If the tumour has gone beyond the bladder wall local excision, however heroic, cannot hope to overtake it.

The patterns in which the needles are arranged are selected so that the rubber patches can fold up for extraction; no pattern greater than 16 sq. cm. is used, but two or more can be applied if necessary. I have not used more than two in any one case.

If for any reason it is thought inadvisable to open the bladder, a bag armed with radium needles can be inserted through a perineal urethrostomy. This method has the disadvantage that the biopsy material is not so valuable when the whole of the surrounding tissue is not also removed.

The bag is tetrahedral, and each of its six edges has attached to it a tube which can carry one or more radium needles. The side bounded by three edges is fairly uniformly irradiated from these edges, and since each side is one-quarter of the total area of the bladder mucosa and is 16·88 sq. cm. it will be seen that the whole area will be 67·5 sq. cm.

The volume of the bag is 36 ml. but the position of the edges is not altered by fairly large variations in over- or under-inflation. It is to a certain extent self-adjusting, as the needles tend to settle themselves round the intrusive neoplasm.

The bag can be inserted either through a large-bore perineal urethrostomy, when a self-retaining catheter of the Malecot type is also used, or it can be placed accurately by hand suprapubically and the inflating tube brought out in the perineum.

One or other of these methods can deal with almost all types of vesical neoplasm, but there is a troublesome wreath of papillomatous tissue which may surround the internal meatus after prolonged use of the Albarran lever during diathermy. Here it seems wise to apply radon seeds arranged round the neck of a Foley catheter so that they exert their effect on the actual bladder neck, where anything as large as a radium needle would feel out of place.

[*February* 28, 1952]

THE following Cases and Specimens were shown:

Hypernephroma, Presenting as Secondary in Talus, Treated by Amputation and Nephrectomy, with a Two-year Non-recurrence.—Mr. B. W. WELLS (for Mr. M. F. NICHOLLS).

Resectable Recurrences of Hypernephroma.—Professor CHARLES WELLS.

Phæochromocytoma.—Mr. E. W. RICHES.

Large Suprarenal Tumour Removed by Right Abdomino-thoracic Incision.—Mr. CLIVE BUTLER.

Carcinoma of Bladder Associated with Metaplasia.—Dr. CUTHBERT DUKES.

Papillary Transitional-Cell Carcinoma of Renal Pelvis.—Mr. J. GABE.

Parathyroid Tumours with Renal Calculi (Four Cases).—Mr. W. R. MERRINGTON.

Double Kidney with Tuberculosis and Ureteric Calculus.—Mr. J. D. FERGUSSON.

Uretero-colic Anastomosis for Multiple Pelvic Injuries (Two Cases).—Mr. H. G. HANLEY.

Recto-vesico-vaginal Fistula with Anal Incompetence. Ureters Transplanted into a Defunctioned Segment of Bowel Draining by Appendicostomy.—Mr. N. J. NICHOLSON.

Total Cystectomy Bearing an encrusted Nodular Carcinoma. This Showed as a Dark Shadow in the Skiagram. The Carcinoma was a Cause of Intermittent Retention.—Mr. A. WILFRID ADAMS.

[*March* 27, 1952]

Urethral Fistulæ from Surgical Trauma [*Abstract*]

By I. H. GRIFFITHS, F.R.C.S.

37 cases of urethral fistula produced accidentally during the course of some surgical procedure in and around the urethra are classified according to the cause:

Male Urethra
1. Instrumentation
   (*a*) Metal bougie 2
   (*b*) Gum elastic bougie .. 1
   (*c*) Cystoscope 2
   (*d*) Rubber catheter .. 11
   (*e*) Gum elastic catheter .. 2
2. Urethral operations
   (*a*) Per urethral resections of prostate or bladder neck .. 2
   (*b*) Internal urethrotomy .. 1
3. Prostatectomy
   Retropubic ⎱
   Perineal ⎰ .. .. .. none
4. Excision of rectum.. 4
5. Injection of piles 1
   Total 26

Female
1. Colporrhaphy 6
2. Bladder neck sling operation .. 2
3. Vaginal cyst 1
4. Obstetric injury 1
5. Impacted urethral calculus .. 1
   Total 11

The most frequent cause of urethral fistula in the male is the misguided passage of urethral instruments. Instantaneous perforations of the urethra usually close spontaneously, but inflammatory perforations associated with an indwelling catheter are more serious and less likely to close spontaneously or by surgical repair. The rubber catheter is potentially the most dangerous instrument, particularly in paraplegia, and cases are constantly occurring which demonstrate the tragic consequences of its mismanagement. Intermittent catheterization, provided aseptic principles are rigidly observed, is most satisfactory but continuous urethral drainage for long periods without a change must be condemned.

Enlargement of the prostate in cases of carcinoma of the rectum presents a special problem, but an effort should be made to avoid prolonged drainage of the bladder by an indwelling catheter by performing prostatectomy before excision of the rectum. The short delay in removing the malignant growth is compensated by a smoother and shorter convalescence. Synchronous removal of rectum and prostate is not without danger of producing a troublesome urinary fistula.

In women, anterior colporrhaphy is the most common cause of urethral fistula and although spontaneous closure does occur urethral repair is usually necessary.

[The full paper will be published in the *Archives of the Middlesex Hospital*.]

The Artificial Bladder — A Short Review [*Abstract*]

By S. GLASER, F.R.C.S.

ATTEMPTS to make an artificial bladder fall into two groups.

Group A.—Preservation of Normal Urethra

In this group an isolated segment of sigmoid colon is used to replace the bladder. The ureters are implanted into the segment of colon and the distal end of the new bladder anastomosed to the bladder neck or urethra around a mushroom catheter. The operation has been successful in dogs and has been used in a human patient.

Group B.—Drainage by Skin Stoma

This group contains several methods and is the group in which the largest number of clinical cases are reported. The basic principle is to implant the ureters into an isolated segment of bowel and drain this by an external stoma, drainage being achieved by intermittent catheterization, indwelling catheter or a Rutzen bag. The segments used have been rectum, cæcum, and ileum. The most favoured method is to isolate the terminal ileum, cæcum and ascending colon, restoring bowel continuity by ileo-transverse colostomy. The ureters are implanted into the large bowel and the artificial bladder is drained through the length of ileum which is brought out to the skin.

Indications.—The operation has been reported for cases of carcinoma of rectum, carcinoma of cervix, carcinoma of bladder, carcinoma of prostate (before introduction of œstrogen therapy), pararectal sarcoma, and vesico-vaginal fistula. The common factor in most of these cases has been the performance of a wide pelvic clearance leaving the problem of disposal of the ureters with the creation

of a colostomy. The artificial bladder has been used to avoid a wet colostomy. In the last group of cases, selected large fistulæ, it may prove an alternative to difficult and repeated operations on unhealthy tissues, e.g. post-irradiation fistulæ.

Choice of method.—In most of the conditions in which the creation of an artificial bladder might be indicated the rectum and colon are also removed and the first group of operations is largely inapplicable. The second group allows for the formation of a new bladder above the pelvic brim and thus permits radical clearance of the bony pelvis.

Advantages.—In a small number of cases coming to surgery it may be necessary to leave the patient with a wet colostomy. This is an unpleasant and relatively uncontrollable stoma. It is suggested that having two controllable stomata, one with clean urine draining and the other passing a solid stool once or twice a day, is far better than having a continual leak of a urinary solution of fæces. The method may also help to avoid the disadvantages of ureterocolic anastomosis. By using an isolated segment the danger of ascending infection is lessened, a direct anastomosis technique is more acceptable and probably biochemical disturbance may be avoided. The artificial bladder deserves consideration as an adjunct in a radical surgical attack on advanced carcinomata in the bony pelvis.

[The full paper will be published in the *British Journal of Urology*.]

Stress Incontinence in Women : Its Cure by Vesico-Urethrolysis [*Abstract*]

By J. H. MULVANY, F.R.C.S.

THE bladder control mechanism has really only one effective component, this being the involuntary sphincter which is sufficient to achieve adequate closure of the bladder. It is reinforced somewhat by the voluntary contraction of the deep transverse perineal muscle lying within the compartment of the urogenital diaphragm; although it must be understood that this muscle cannot control a stress incontinence, its value in this respect being at most the power of retaining for a brief space within the upper urethra a few drops of urine which may escape into it. In regard to the levator ani muscle, it is unlikely that this is concerned in the development of stress incontinence in view of the fact that it may be completely excised without affecting urinary control. Hence it is doubtful whether its anterior approximation below the bladder, forcibly achieved under anæsthesia, can remedy the inefficiency of the internal sphincter. As for that band of striated muscle sometimes referred to as the levator urethræ or anterior sling, this is really part of the deep transverse perineus which varies in relation to the urethra according to the level of the plane of section. It is neither a levator, nor a sling, nor a sphincter.

Stress incontinence takes two forms. One develops from simple weakness of the internal sphincter and may appear at three phases of life. Thus, it is seen in childhood from inherent or neuromuscular defect, in mid-life due to involutional hypotonia or in senescence as a result of slow atrophy. The second type is associated with damage to the anterior or subpubic portion of the triangular ligament and stretching of the internal sphincter during parturition. Lack of support allows the bladder neck to drop, its upper surface, however, remaining held up by the pubovesical ligaments and other attachments. Thus, the sphincter ring, already weakened, is widened and its efficiency further impaired.

Vesico-urethrolysis is a simple measure aimed at freeing the sphincter area in a good part of its circumference from any adhesion which tends to keep the muscle in a stretched and ineffectual position. In this way the sphincter is enabled to retract and regain its efficiency. It is of value in all types of stress incontinence without urethrocele or marked cystocele. After exposure of the retropubic space, a finger is swept around the bladder neck freeing it and the abdominal urethra from all visible fascial connexions above the pelvic floor. The pubovesical ligaments may be cut or left intact according to the degree of lysis desired. A vault prolapse, if associated, should be treated by subperitoneal suture of the uterosacral folds. For this, the abdominal cavity must be opened and the peritoneum on the back of the cervix incised. It is advisable to define the central ends of these ligaments for $1\frac{1}{2}$ in. in order to ensure satisfactory tightening of the cardinal and pubocervical bands which are also part of the same fascial expansion. A small cystocele can also be dealt with from above. One very valuable feature of a vesico-urethrolysis, in view of its simplicity, is that it may follow on any pelvic operation, no matter how tedious or difficult, when stress incontinence is associated. Even the presence of a urethrocele is not a contra-indication as many of these are improved and certainly no harm will follow.

Stress incontinence with urethrocele should be treated from below by a routine repair. In this operation, the percentage of success will depend upon the degree of mobilization given to the sphincter and the form of support to retain it. The first step, therefore, is to free the triangular ligament from the vaginal wall as far laterally as the pubic rami and forward to the transverse pelvic ligament. In this way, a deep para-urethral fossa extending two-thirds around the urethra is formed. Lack of thoroughness here may lead to disappointment in what has otherwise been a technically perfect operation. Having thus freed the triangular ligament, the relaxed membrane with attached sphincter area is pushed up into the apex of the subpubic angle and a stitch applied to hold it in this position. A mattress

suture is passed through the vaginal wall close to the pubic ramus taking up the adjacent parts of the bulbocavernosus and triangular ligament on each side. When this suture is drawn together, these structures are approximated creating a firm support to the bladder neck and ensuring satisfactory relaxation of the sphincter.

Vesico-urethrolysis in properly indicated cases is followed by immediate relief, although some degree of urgency or frequency may persist for a few weeks. The remarkable success of the measure suggests that part of the benefit following the vesico-urethropexies or fascial procedures may be due to the free mobilization of the sphincter which their application entails. With an intact triangular ligament, however, a vesico-urethrolysis is practically assured of success.

Some Observations on the Treatment of Calcifying Renal Tuberculosis
[*Abstract*]
By HOWARD G. HANLEY, M.D., F.R.C.S.

A STUDY of a personal series of 87 nephrectomy operations for tuberculosis showed radiological evidence of calcification in 37 cases or 42·5%.

In the past it has been the accepted teaching that calcification was evidence of healing, and that if a calcified non-functioning or symptomless kidney was discovered accidentally, it should be left alone. This view is contested and a plea is made for the removal of such calcified tuberculous kidneys as soon as conveniently possible after their discovery. To begin with, the term calcification is misleading. There is rarely, if ever, true hard calcification, but there is always caseation, and it is the deposition of calcium salts in this caseous material which produces the radiological shadow upon which we base our diagnosis of calcification. There is no more evidence of healing in an area of caseation containing calcium salts than in one without. Even when the kidney is completely sealed off the term "auto-nephrectomy" does not make sense. The kidney is still present and is generally anything but quiescent. In fact a careful examination of 13 "calcified" closed or quiescent kidneys discovered accidentally during routine investigations for other causes, showed the presence of living tubercle bacilli in 10 (*see* Table I).

TABLE I.—37 NEPHRECTOMY OPERATIONS FOR TUBERCULOSIS WITH CALCIFICATION

14 patients had symptoms for less than nine months
    2 under nine months
    9 under six months
    3 under two weeks
5 presented as "Pyelitis of pregnancy"
    (4 without any previous symptoms)
15 calcified kidneys discovered accidentally during routine investigations—all
    without urinary symptoms. 13 kidneys were "closed" and non-functioning,
    but from 10 of these, living tubercle bacilli were isolated after operation
3 presented without urinary symptoms, but with complications due to a breaking-
    down calcified lesion.

It is quite untrue to say that such symptomless calcified kidneys may be safely left in situ, for whether there are calcium salts in the caseous material or not, the condition is still a tuberculous pyonephrosis, and should be treated as such by nephrectomy or nephro-ureterectomy, since we do not know whether or when they will break down and liberate tubercle bacilli into the lower urinary and genital tracts.

14 patients in this series had symptoms for less than nine months, but the calcified caseous material must have been present for much longer than the duration of these symptoms. The presence of this calcification for long periods in the absence of any urinary symptoms is of considerable interest. It may mean that tubercle bacilli can be passed through the bladder for perhaps years without producing any obvious effects, or it may equally well indicate that there really is an attempt at healing, which prevents the escape of bacilli from the lesion.

The fact remains that we cannot tell what causes the sudden liberation of tubercle bacilli from a closed calcified lesion, or when it will occur, so that to leave such a kidney in situ is not in the patient's best interests.

Five calcified kidneys were discovered during routine investigations for pyelitis of pregnancy. These are anxious cases to treat and much suffering would have been avoided if they had been fortuitously discovered before pregnancy (as, in fact, one case was) and treated by nephrectomy.

A further 3 cases presented with perinephric abscesses due to rupture of symptomless "calcified" lesions. In one of these the calcification was known to have been present for two and a half years.

These 37 kidneys were removed without any complications due to the operation, without any cases of sinus formation and without any deaths; so that while there are many reasons in favour of removing such organs, there would appear to be no reasons for not doing so.

Section of General Practice

President—J. D. SIMPSON, M.D., M.R.C.P.

[*March* 19, 1952]

DISCUSSION: DERMATOLOGY IN GENERAL PRACTICE

Dr. S. R. Wood: We are all increasingly aware nowadays of the part that psychological factors play in precipitating, aggravating or perpetuating bodily disorders. In the skin the transient reversible changes wrought by neurovascular mechanisms under emotional stimuli are plain for all to see. If the blushing and flushing of the rosaceous patient later leads to permanent telangiectasia and other secondary structural changes, one must concede that chronic psychological stress may facilitate other derangements of function and structure to which the patient is constitutionally prone. There is undoubtedly a strong psychological component in the production of itching, the major symptom which is both the cause and effect of skin disorders. The good general practitioner has little to learn from the cult of "psychosomatic medicine" and knows the value of a few minutes spent in unveiling hidden anxieties, guilts and fears, followed by positive reassurance and fortified by a mild sedative or placebo.

This approach is nowhere more valuable than in the field of dermatology. For example, comparatively few skin diseases are contagious; it is not enough to tell the patient—one should make sure that his family, relatives, employers and even, in certain cases, his landlady understand it. Too often the patient with psoriasis or chronic eczema wanders from job to job or from lodging to lodging, a victim of the stigma of uncleanliness.

Young men, especially those with extensive rashes or with lesions around the genitalia, *do* need to be told that the trouble is not venereal and the adolescent with acne vulgaris that it carries no sexual stigma. The fear of cancer, however irrelevant, should not be forgotten.

It is indefensible to tell young patients with psoriasis that their disease is "incurable" or young girls with acne vulgaris that the spots are of no consequence, statements which can cause an incalculable amount of mental trauma in sensitive young patients.

The patient's attitude to a chronic complaint like psoriasis is all important and provided that the skin trouble is neither incapacitating nor disfiguring he should be encouraged to regard symptomless patches not as a disease but rather as a constitutional "condition"—relapses and exacerbations being his own personal way of reacting to "the slings and arrows of outrageous fortune". We are all aware of the close link between active psoriasis and upper respiratory tract infection—and the rheumatic states, including gout; we know that psoriasis may worsen at times of physiological imbalance, and that it may appear at sites of local trauma; so that this concept, comforting to the patient, may yet not stray too far from sound physio-pathological principles.

Acne vulgaris.—Beginning as an expression of emotional and endocrine instability in a seborrhœic patient, it is aggravated by inadequate toilet of the scalp and skin, in some instances by self-mutilation, by dietary faults and by focal infection. Locally, an efficient toilet regime at night, especially important in young girls who use cold creams instead of soap and water, is followed by a mild astringent lotion such as 2% sulphur and 15–20% spirit in calamine lotion thickly applied and allowed to dry. A spirit-soap shampoo can be used weekly to which 1–2% resorcin can be added in dark-haired people, and a liniment such as lot. acidi salicyl. et hydrarg. perchlor. (N.F.) is applied to the scalp once daily, no greasy preparations being allowed.

Excess carbohydrate and cooked fat, including chocolate, are eliminated from the diet and focal sepsis is removed on its merits. Œstrogens for the girls and thyroid extract for the heavy types of both sexes may be given over long periods. These patients are self-conscious and often emotionally restricted—almost sullen—in manner. When treatment begins to take effect they should be encouraged in the social activities which Youth Clubs and similar organizations offer. When progress ceases, or in severe established cases of several years standing, one needs the advice of a consultant on the question of ultraviolet light or X-ray therapy. It cannot be too strongly emphasized that scarring is the unfortunate result of untreated acne and that X-rays, in carefully controlled doses, are a most valuable form of treatment. Many patients fail to distinguish old scarring from present activity and unless this is pointed out are apt to be disappointed with the results of treatment.

Impetigo, though less common nowadays, is often badly treated. When first seen the staphylococcal type is frequently bullous, or the bullæ may have ruptured to form moist ringed lesions which may, despite their rapid evolution, be misdiagnosed as ringworm. Nothing will spread this type of infection quicker than greasy applications, yet the popular ung. hydrarg. ammon. dil. still holds the field. Sulphonamides are now widely recognized to be potential sensitizers, and local penicillin is often not only equally dangerous but useless in the presence of resistant strains of bacteria. Yet according to Professor Dunlop's recent survey of prescriptions on form E.C.10, 10% of the total prescriptions for sulphonamides were for skin preparations and no less than 40% of the prescriptions for penicillin were for penicillin ointment. It is still true that the mildest anti-bacterial agents will cure so long as they do not irritate or interfere with the remarkable self-disinfecting power of the skin.

Iron and vitamins by mouth are valuable and in stubborn or recurrent infections one should remember the possibility of auto-inoculation from a nasal focus or from an intermittently discharging ear.

In all eczematous conditions it is best to avoid the unqualified term "Dermatitis" with its strong industrial connotation; the National Insurance Act, Section 24b, uses the term "Inflammation of the skin." Where a rash is clearly non-occupational one should say so at the outset. In many cases of eczema the decision is difficult, but even here the practitioner, with his previous knowledge of the patient and of his work has the advantage of the consultant who must, by questioning the patient about his working conditions, inflame the suspicion that the rash is occupational and may sow the seed of "Compensation" in receptive soil.

Whatever the cause, anxiety over retaining a good job, the conflict between good money and hazardous work, or dissatisfaction with the job, may all be factors in perpetuating invalidism and call for a frank assessment of the case, ideally in collaboration with the Works Medical Officer.

· *Use and abuse of the consultant service.*—Even though the best treatment for a conspicuous pigmented mole may be plastic surgery, or for a rodent ulcer radiotherapy, the dermatologist should be the first court of appeal. On the other hand, patients with persistent local or general pruritus for which no organic cause is apparent and in whom the skin is normal, apart from the effects of scratching, should go to the general physician or the psychiatrist. Such cases usually carry a bad prognosis. The outlook is somewhat better for those women with pruritus vulvæ who present with definite skin or mucosal lesions of inflammatory type. In these one should look particularly for evidence of seborrhœic dermatitis or, in the case of mucosal patches, for fungus infection. Provided that there is no irritating vaginal discharge or urinary stress incontinence it is probably wiser to refer these patients to a dermatologist. Gynæcologists are prone to regard these conditions as leucoplakic and consequently potentially malignant, and many a young woman with an infective dermatitis has been subjected to the mutilating operation of vulvectomy.

Principles of management and treatment, with special reference to eczema.—Organic disorders of the skin—congenital, neoplastic, the specific granulomata, &c.—are comparatively uncommon and the majority of skin conditions met in general practice are functional in that, in the first place, they represent the response of a sensitive skin—the target organ—to stress both from without and within and secondly, the accompanying structural changes are largely reversible. Because we are ignorant of the mechanism whereby, for example, psychological upset is translated into eczema, treatment must be mainly empirical. But sound empirical treatment does not mean, "This is a new drug, let us try it and see if it works"—on the contrary it involves not only a knowledge of the natural history of the disease, but also recognition of the whole constellation of possible ætiological factors and their appraisement in the individual patient. Thus, seborrhœic dermatitis is one of the commonest and often one of the most intractable of skin disorders. It is essentially due to the transformation of harmless skin saprophytes into pathogens capable of evoking the eczema reaction through the mechanism of autosensitization. Constitutionally the typical thin dark-haired seborrhœic has lined angular features, the complexion is muddy or greasy with large sebaceous orifices and the vasomotor apparatus of the skin is active; there is a tendency to red-rimming of the eyes and to fronto-temporal and vertical baldness in the male, who is often dyspeptic and edentulous at an early age; reserved, almost rigid in manner yet tense and anxious beneath the mask, meticulous in his habits, a perfectionist in his work.

Upon this diathesis a number of "stress factors" have been shown to operate and treatment must aim to correct, counteract or modify these.

The most important are:

(1) Climatic and environmental, for example warmth and humidity which increase sweating and the growth of micro-organisms; contact with external irritants and sensitizers.

(2) Endogenous bacterial infection.

(3) Nutritional and metabolic factors, whereby dietary faults affect the water and mineral salt content of the skin; intestinal mucosal defects may cause secondary vitamin deficiencies; or an abnormal bowel flora may lead to absorption of harmful products of protein breakdown.

(4) Endocrine factors.

(5) Psychological factors, particularly anxiety and conflict with little or no chance of emotional lease.

These precipitating factors may be single but are more often multiple and their effect is to lower the threshold of resistance of the skin and mucosa first to organisms normally saprophytic, which thereby become pathogens and initiate the seborrhœic dermatoses and the chronic catarrhal conditions of the upper respiratory tract; secondly to pathogenic cocci so that pustular follicular eruptions result; and thirdly to external allergens, thereby predisposing to contact eczema.

I have found the following principles useful in the management and treatment of eczema.

(1) Each case should be considered as a potential medical problem—the constitutional side should be assessed with special reference to a past or family history of allergic illnesses, the internal factors investigated and contact with sensitizing agents both in the home and at work enquired into and, if possible, investigated on the spot.

(2) Remember the fundamental importance of rest, for the patient and for his skin.

(3) Draw up a plan of local and general treatment which the patient can follow:

(i) Prescribe well-tried bland remedies such as calamine lotion, zinc and ichthyol cream or Lassar's paste. Avoid local chemotherapy and local anæsthetics. Avoid fungicides and proprietary remedies contained in elegant greasy bases of unknown composition.

(ii) Be particularly cautious in the active evolving stage of the rash.

(iii) Do not change local treatment too often.

(iv) If many remedies have failed withhold everything for a few days—over-treatment is perhaps the commonest fault.

REFERENCE

DUNLOP, D. M., HENDERSON, T. L., and INCH, R. S. (1952) *Brit. med. J.* (i), 292.

Dr. F. Ray Bettley: It may be of interest first of all to enquire into the relative incidence of the various skin diseases. Fig. 1 shows the incidence of different dermatoses among 18,273 new patients seen at St. John's Hospital for Diseases of the Skin during 1951. It is striking to observe that 36% of all these cases were diagnosed as belonging to the "eczema-dermatitis" group. Next in order of frequency comes seborrhœic dermatitis, and the thirteen most frequent diseases taken together make up no less than 78% of the total. The rarer conditions indicated at the bottom of the chart represent 16% of the total number of cases and comprise 313 different diseases. These, of course, are specialist dermatology; their diagnosis is often made on clinical grounds alone and it is clearly beyond the

SEBORRHOEIC DERMATITIS   ECZEMA GROUP
PYOCOCCAL
WARTS
ACNE
PSORIASIS
RINGWORM
NEURODERMATITIS
URTICARIA
ALOPECIA AREATA
ATOPY
NAEVI
ROSACEA
LICHEN PLANUS
CARCINOMA
LUPUS ERYTHEMATOSUS
INTERTRIGO
SCABIES

0% 1 2 3 4 5 6 7 8 9 10   20   30   36
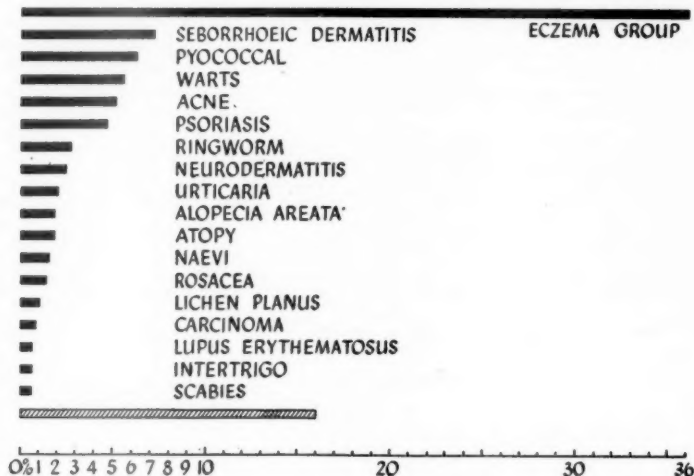
FIG. 1.

scope of the non-specialist to familiarize himself with them. On the other hand, most of the diseases named on the chart are not only sufficiently common to be familiar in general practice but, for the most part, are diagnosed on clinical grounds requiring no special facilities for investigation. Their treatment also is for the most part well within the scope of general practice since it requires little in the way of special equipment or technique. In many of these disorders emotional or psychiatric factors are important, and I believe that the general practitioner is often in the best position to deal with this aspect of the patient.

Among the large group of eczematous eruptions, contact eczema or exogenous dermatitis forms a large part. Indeed, the more one looks for contact sensitivity as a cause of eczema, the more one finds it, and the proportion of those cases which have to be labelled "idiopathic" or "constitutional" dwindles very considerably.

I would like here to record some investigations on a remarkable case of this sort:

Case report.—A medical practitioner, aged 53, first developed eczema of the face and hands in April 1942, while serving in the Royal Air Force. Because of this complaint he was invalided from the service in December 1942. Attacks of the eruption have continued ever since.

When I first saw him, in 1942, he told me that attacks of dermatitis would result whenever he handled rubber; contact with the tubes of his stethoscope brought on an eruption on the sides of the neck, and his wrists and hands had been affected when he rested them on the rubber-padded arms of a cinema seat. He also gave the almost incredible history of the occurrence of an attack after he had handled blankets on which rubber hot-water bottles had lain, though he had not handled the hot-water bottles themselves. He had also experienced an attack by handling the steering-wheel of his car after his car had been driven by a mechanic who had been handling a tyre immediately before. This remarkable history of indirect contact was later proved to be accurate.

Patch-tests with a wide variety of different types of rubber gave positive reactions only with those rubbers which contained *benzthiazole*. He was also sensitive to chlorbenzthiazole but not to several other allied substances with which he was tested. His extreme sensitivity was borne out by establishing a positive patch-test when benzthiazole was applied in a solution of one part in 10^{14} (100,000,000,000,000). Further, the effect of indirect contact was verified—three small pieces of lint were put loose in a matchbox with a small piece of rubber for three days; at the end of this time it was found, under suitably controlled conditions, that they elicited a positive patch-test.

In the course of other patch-tests, we found that it was necessary for the patch to be in contact with the skin for at least fifteen minutes for a positive reaction to be eventually elicited and that half to one hour elapsed between the commencement of contact and the beginning of burning and itching.

Antihistamine drugs given by mouth had no effect.

This case is described in detail because of the extremely high degree of sensitivity which the patient presented, so that indirect contacts would bring on an attack. It will be readily appreciated that had the doctor himself not been able to contribute to the history and investigations, the underlying cause might well have escaped detection. Even now, the doctor tells me, attacks will occur although he is most careful to avoid them; it is only in a small proportion of these that he is able to identify with any certainty the mechanism by which the attack was induced.

Specific desensitization in a case of this kind is not likely to succeed and, in view of the untoward reactions which might occur, I am inclined to think the doctor was wise in declining my offer to attempt desensitization.

[Case histories were then briefly presented of patients suffering from contact eczema from the following causes: Antihistamine creams (2 cases); a proprietary antiseptic ointment; hatband; plastic spectacle frames; uncreasable neck-tie; nickel-plated suspender buckles; acrylic resin; an alkaline cleanser; french polish.]

Contact dermatitis, I believe, is essentially a disorder with which the general practitioner can and should deal. The diagnosis and investigation seldom require special apparatus and the only technique is that of the patch-test. It is essentially a disease of environment and nobody is in a better position to know the patient's environment than his own family doctor. In some difficult cases a great deal of help has been obtained by paying a visit to the patient's home but, for the most part, the specialist knows the patient's environment only by what the patient tells him. In having to rely to such an extent on the patient's history, a specialist is at some disadvantage compared with the family doctor who can so easily visit the patient's home and see for himself the surroundings and possible sources of allergic sensitivity.

I would like to emphasize that the treatment of contact eczema is hardly likely to succeed until the contact has been identified; and that when it is identified its removal will enable recovery to take place with the simplest of treatment in the great majority of cases.

Section of Orthopædics

President—PHILIP WILES, M.S., F.R.C.S.

[*March* 4, 1952]

Surgery and the Surgeon

PRESIDENT'S ADDRESS

By PHILIP WILES, M.S., F.R.C.S.

IN this short Address I shall try to formulate some of the ideas that have guided me in practice and discuss some of our immediate problems. It is perhaps right to start by thinking about the nature of surgery, because surgery is a peculiar business, and for this reason: Every operation must be a destructive proceeding. It may be performed for a constructive purpose but the mere fact of cutting the tissues destroys them to a greater or lesser extent and the body has to go to great trouble to heal the damage. And when a part is completely excised, the body has to adjust itself to be able to carry on its existence without that part. To do harm in order that good may result is ordinarily regarded as an unsatisfactory basis for action, and some people would argue that it is never justified. However, there are circumstances in which it appears to be so much the lesser of two evils that no other course is possible. A surgical operation is an example of causing damage in order that good may result, and one which all of us who are surgeons must believe is justified. But we must appreciate that the procedure is fundamentally unsound or we will not be impelled to seek a more rational method of doing good. Much of our thought and of our energy is devoted to finding new and better operations. This may be permissible as a temporary expedient, but we should be conscious of the fact that the very need for operation is in itself an admission of failure and seek better methods of preventing injury and disease, and better methods of treatment. In an ideal world there might be no need for surgeons at all, and however important surgery may be now, it should be the aim of all doctors, including surgeons, to limit and ultimately to abolish it.

THE CONTRIBUTION OF ORTHOPÆDIC SURGERY

Some little progress has, in fact, been made towards the control of surgery in a few directions, but nevertheless the number of operations performed continues to increase by leaps and bounds. Orthopædic surgery has developed even more rapidly than many other branches, but not only in the sense of bigger and better and safer operations. It has developed in another way as well and it is this that has been the driving force which has compelled the growth of the specialty, and which represents its major contribution to surgery as a whole. I am referring to the importance orthopædic surgeons place on the preservation and the restoration of function, and the preservation of the outward appearance of the body. It happens that we deal with parts of the body that are essential to almost every phase of human life whether eating, working or playing. Of course other parts are really just as important, but often in a less obvious way. A man with an ulcer of the stomach can have the whole organ removed and still retain his full earning capacity and even enjoy his meals, provided they are not too big. In contrast, an infected hand, far from being amputated, as can be a stomach, must be treated with such meticulous care that no disability whatsoever remains. Perfect function must be

restored to that hand. Moreover, we deal for the most part with those parts of the body that can be seen and felt and watched working. Most men are more conscious of their limbs than of the hidden organs, and they insist that the parts they can see do work properly. If the stomach were on the surface of the body, a patient might resent its amputation as much as the loss of a breast.

What does this business of preserving function actually mean, and what does it involve? Firstly it requires a special attitude of mind on the part of the surgeon and all those working with him, and this must quickly be extended to include the patient without whose whole-hearted co-operation there can be little success. Secondly it means that every part, every structure, damaged or undamaged, must be made to function from the earliest possible moment and it must function by the conscious effort of the patient. Only in this way can tissues that are damaged and œdematous as the result of trauma, of infection, or of injury inflicted by surgical procedures, be prevented from deteriorating. To this end, the co-operative efforts of the whole orthopædic team must be directed. This is the reason we have developed such wonderful physiotherapy and rehabilitation services. British orthopædic surgeons lead the world in this respect, and our rehabilitation and social services are far in advance of the general run in other countries. Without such aid many of the operations now performed would be useless and no surgeon of repute in this country will undertake reconstructive surgery unless he is backed by a first-rate physiotherapy service. Such a service is of prime importance to the orthopædic surgeon. Operation is but one incident in treatment and pre-operative and post-operative treatment carried out by physiotherapists both in the wards and in the out-patient department are an integral part of the whole procedure, and it *must* be under the full control of the surgeon. There is a real need for a specially trained doctor to be responsible for the organization of the physiotherapy department and the care of the expensive machinery that fills it, and this can very properly form part of the duties of the specialist in that group of complaints now known as "rheumatic" diseases. But this in no way reduces the responsibility of the surgeon to his patient. Unless the surgeon can ensure that treatment is carried out according to his prescription he is in no position to advise the patient in the first place that treatment will help him—and that is so whether operation happens to form part of the treatment or not. Moreover, the physiotherapists must owe their loyalty to the surgeon or else the co-operation that is essential to success will not be achieved.

PROBLEMS OF ORTHOPÆDICS

Orthopædic surgery differs from some other divisions of surgery in that it is concerned less with the saving of life and the eradication of disease than it is with the relief of symptoms. When treating a complaint such as osteoarthritis our success is judged by the extent to which we succeed in ridding the patient of his pain and relieving his disability. We have, therefore, a very particular responsibility when advising a patient to undergo an operation. There is one principle that must guide us. We must be confident that the patient will leave our hands in a better condition than when he came into them. Remembering the damage and mutilation our operation will inflict on the body of the patient, we must be reasonably sure that the resulting improvement will be more than commensurate with the ordeal he must undergo. In coming to a decision there are three questions we have to answer, and this may be difficult because we are dealing not with life and death matters, but with the relief of symptoms.

(1) *What are the risks of operation?* When a major procedure is concerned there is a definite risk to life, and there is a risk of a poor end-result. Even with a smaller operation there is a real risk to both life and limb. Even an operation for bunions carries both these risks. I myself have lost a patient from pulmonary embolism after an operation for hallux valgus. There is also a risk that the operation will be a failure and that risk is not so very small. There is a policeman walking around this country who lost both his big toes from gangrene after an operation for the cure of bunions. During the past year I have amputated the big toe of one patient and arthrodesed the metatarsophalangeal joints of two others as the result of operations for hallux valgus that have been badly performed. I do not pretend that in these days the risk of a catastrophe following such an operation is very great. The operations already quoted were not mine in the first place and I cannot calculate the percentage risk. But at the moment there is a man in my wards who may lose his foot as the result of a Keller's operation. This is the only real failure of mine since the war. I personally perform, or am responsible for, at least 100 such operations a year, so the risk of a bad result may be something like 1 in 500 or 600. This seems a fair risk to take, but curiously enough it is the real gamblers who, as patients, dislike taking it most. The punter should know how seldom he wins at 100 to 1, but he bets with the object of winning, he anticipates winning, so when the position is reversed he anticipates being the 1 that goes wrong as against the 99 with good results. If there is a moral to this, it is that the fear some people have of operation should be respected. When we meet such a fear, whether it seems to us rational or irrational, we must remember that we are trying in such cases, not to save life, but to relieve symptoms, and it is the patient, not we, who must weigh the risk and balance his apprehensions against a possible gain.

(2) *What is the cost to the patient in time and suffering?*—This question is a little easier to answer except, of course, for the risk of complications such as those already mentioned. But it must be remembered that the disruption in the life of every household is considerable when any one member is away; it is a very grave matter in a low-income household when the breadwinner or the mother is absent for six months or more. Consider an operation for a prolapsed intervertebral disc in, say, a man having low back-pain and occasional sciatica but who has managed to keep at work. It is easy to say "come along into hospital and I'll put you right in no time", but what is the cost to that man and his family? How often have you seen that same man six or twelve months after operation and still not back at his old work? The risk may be well worth taking when the man is completely incapacitated to start with, but perhaps not so in other circumstances. The right time for operation is when the patient knows the risk and is willing, indeed more than willing, when he is anxious to take that risk. I have had sciatica, and so have some of you. At what point will we submit ourselves to operation?

(3) *What benefit may the patient expect to receive from operation?*—This is much the hardest question of the three because it involves two things, assessment of the severity of the symptoms, and estimation of the probable functional result; both depend on a number of imponderable factors. Consider the problem in relation to arthroplasty of the hip, an operation with a small risk to life, but requiring a prolonged period of after-care. The functional result depends on the personality of the patient, on his threshold of pain, and on the extent to which he will co-operate in after-treatment; it depends on the condition of the bone and the muscles, and on such factors as post-operative fibrosis and new bone formation. All these things are so uncertain we can do no more than hazard a guess, a guess that with increasing experience may become more accurate, but it remains a guess. We are therefore compelled to make a very conservative estimate of the probable functional result to set against the patient's present state.

Estimation of the patient's present state really amounts to the assessment of the severity of his pain and the extent of the disability. Pain is notoriously difficult to estimate, but it is usually possible to get an idea as to the extent to which it is a major disabling factor by closely questioning the patient about the commonplace things of life, like going upstairs, doing the family shopping, or sitting, and sleeping. Loss of function can be gauged more objectively, but caution is still necessary because different people have such very different requirements, and they differ, too, in their ability to adapt. It is necessary to inquire in great detail into just what things the patient most wants to do that he cannot do at present, and see if it is likely that after the operation he will be able to do these things.

THE TRAINING OF SURGEONS

The problems of the orthopædic surgeon differ in degree rather than in kind from those of the general surgeon and the other specialist surgeons. His fundamental training must therefore be that of a surgeon. A good training in the actual techniques of orthopædic surgery is not so hard to get, but what of the basic surgical training? The general training, traditionally, has been given by the surgeon in charge of the general wards of our teaching hospitals, and the high standard of surgery, both general and specialist, in this country is sufficient testimony to the success of their work. There has, however, been a great change in the pattern and character of surgery during the last two or three decades owing to the rapid development of specialization. This has given rise to two problems that must be solved in the near future: how is the surgeon in training to obtain a sufficiently varied experience now that the general surgical wards no longer contain a representative cross-section of surgical practice? and how should the surgical beds of a teaching hospital be divided to give sufficient representation to the specialties and yet maintain a proper balance?

(1) There are four stages in the training of a surgeon. The *first* is the undergraduate stage and the *second* the resident, or houseman, stage. These two stages are normally carried out in the general surgical wards of a teaching hospital and they are common to all who intend to become practising doctors. It is during these stages that the student learns such elements as the nature and treatment of inflammation, the response of the body to injury, and the habits of tumours both malignant and benign. In these wards, too, he becomes acquainted with *a few* of the diseases for which surgery can offer a remedy. There also he sees something of the dangers as well as the successes of surgery, and should come to understand something of surgery as seen through the eyes of the patient. He should learn when it is appropriate for an individual patient to submit to operation, and when it is not. It was natural that this teaching should have been carried out in the general surgical wards, indeed it could not be elsewhere at a time when these wards comprised almost the entirety of the surgical beds of the hospital. But the work of general surgeons in teaching hospitals has changed and the patients in their wards have become and still are becoming less and less varied in their complaints. It is common to find a majority of beds occupied by people with diseases affecting, say, the genito-urinary, or the cardiovascular, or the biliary systems, or perhaps the thyroid or the lower bowel.

The great surgeons and the great teachers of the past were general surgeons. I believe their success

was due as much to their great stature as surgeons and as teachers as to the fact that their wards contained a wide variety of clinical material. Of course, there are many great general surgeons to-day, but there are also surgeons in the specialist branches who rank equally as teachers, as doctors and as craftsmen.

The two chief reasons why the early stages of surgical training are in the hands of the general surgeon are no longer good reasons. That is to say there is no longer a greater variety of teaching material in general surgical beds than in some specialist beds, and there are now specialist surgeons of the proper calibre for the work of teachers. The general surgeon is therefore no longer entitled to the exclusive right to the honour of teaching surgery.

The *third* stage in a surgeon's training, that of general surgical training, is now known as the registrar stage. This, of course, has been undertaken up till now by the general surgeon, but it is becoming increasingly difficult to give a proper training as general surgery becomes more restricted in scope. The advantages of serving this period in apprenticeship to one chief are great but it may be that they are becoming outbalanced by the disadvantages accruing from the accompanying restriction in experience. It seems that we are being driven, *faut de mieux*, to a rotating system whereby the trainee will spend a time in a number of different surgical departments. And because of economic pressure, the time with each may be lamentably short.

The *fourth* stage in training, that of specialist training, is by far the easiest to arrange at present, and I have only these comments to make. It should be carried out as far as practicable at a university centre where undergraduate or postgraduate teaching is undertaken because only there will the trainee be subjected to a continual fire of criticism. And the maximum time should be spent with one chief.

(2) The second problem raised by the increase in specialization, and it is parallel with the first, is the increased demand for more in-patient accommodation for the various specialties in teaching hospitals. During the last decade the bed accommodation for the specialties has increased but little, whilst the general beds have gained to the extent that specialist cases are no longer admitted to them. Specialist work is therefore insufficiently represented at teaching hospitals of the university centres, and this must be to the detriment of students of all types. Experience has shown that this difficulty cannot satisfactorily be overcome by the acquisition of beds at peripheral hospitals and another solution must be sought.

I believe that both problems, that of providing a comprehensive training in surgery, and that of restoring a balance of beds more accurately representing present-day surgical practice, would be helped if in appropriate cases the specialist surgeon were given charge of beds now allotted to general surgery, and the specialist joined in the basic training of undergraduate and post-graduate students. The appointment as teacher should be given to the man most suited to fill it, and the specialist surgeon can be as well fitted as any other to teach the fundamentals. These are common to several branches of surgery, as are the general methods of examination of the patient, and it should not matter if they are demonstrated with particular reference to one set of organs or another. With such a system the undergraduate student who has to spend six months in surgical wards might find himself with three months of "general" surgery, and three months with a genito-urinary, or a proctological or an orthopædic surgeon who was using his special cases to illustrate surgical principles. The student who spent half his time in the orthopædic wards would indeed be fortunate because the types of case he was taught on would be more representative of those afterwards met in practice—at my hospital more new patients attend the various clinics for which the orthopædic department is responsible than attend the clinics of all the four general surgical firms added together.

Section of Medicine

President—Sir ALUN ROWLANDS, K.B.E., M.D., F.R.C.P.

[*April* 22, 1952]

A Diagnostic Test in Obliterative Arterial Disease of the Legs

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OBLITERATIVE or occlusive arterial disease may affect the blood supply to the legs without causing symptoms, either because it is not sufficiently severe, or because some other disease such as angina pectoris partly restricts the patient's activity. If symptoms are produced, they may be:

(1) *Intermittent claudication*: Pain in the legs brought on by walking and eased by rest, and causing limping. As well as pain a feeling of tiredness in the legs is not uncommon.

(2) *Gangrene and pain* in the legs at rest.

Intermittent claudication and gangrene may present as separate problems in any individual. Possibly the physician more often sees the patients with intermittent claudication, and the surgeon those with gangrene.

Special methods of investigation usually add little to the careful clinical study of the patient with occlusive arterial disease affecting the blood supply to the legs.

In the clinical examination of such patients there are many more signs to indicate the presence of occlusive arterial disease, than in the analogous condition of angina pectoris. This is fortunate as the history is probably more often misleading; such conditions as osteoarthritis of the joints, meralgia paræsthetica, foot strain and possibly prolapse of an intervertebral disc may be included in the differential diagnosis of intermittent claudication.

The diagnostic test to be described is especially applicable to the early case, in which the more obvious later signs of the condition may not have developed. These latter are:

(1) The skin is dry, and poorly nourished; it sweats little and is deficient in hair. The nails too are atrophic, but these should be differentiated from those affected by fungi.

(2) The muscles of the calf, and sometimes those of the thigh and buttock, depending on the level of arterial obstruction, may be wasted.

(3) Colour changes: The limb, and especially its periphery, may be blue or pale. It may blanch on elevation. If exercised by dorsiplantar flexion with the patient lying horizontal and the leg elevated to 45 degrees, the soles of the feet may become pale in contrast to their normal pink colour. It was in 1907 that Dr. F. Parkes Weber first described this test to the Royal Society of Medicine.

(4) Temperature: As the palpating fingers are passed distally the limb is found to become colder; an indication of the level of arterial obstruction may be obtained in this way.

(5) Tenderness to palpation of affected muscles may be found.

AUG.—MED. 1

(6) Pulses may be absent or diminished in amplitude, but it is well to remember that they are some-times so in health.

All these signs may be absent and the physician may require the help of special investigations to establish a diagnosis. Such investigations are diagrammatically represented in Fig. 1; the list is not a comprehensive one. Arteriography and aortography are useful in the diagnosis and particularly in the location of arterial occlusions but neither is a simple procedure. We have not found that oscillometry itself adds much to a careful clinical examination. Plethysmography is of

OCCLUSIVE ARTERIAL DISEASE

ARTERIOGRAPHY
TONOSCILLOGRAPHY
AFTER EXERCISE
OSCILLOMETRY

REDUCED SKIN BLOOD FLOW   REDUCED MUSCLE BLOOD FLOW

MEASUREMENT OF
SKIN TEMPERATURE   FOOT   CALF
HISTAMINE TEST
FLUORESCEIN TEST

?

PLETHYSMOGRAPHY

GANGRENE   INTERMITTENT CLAUDICATION
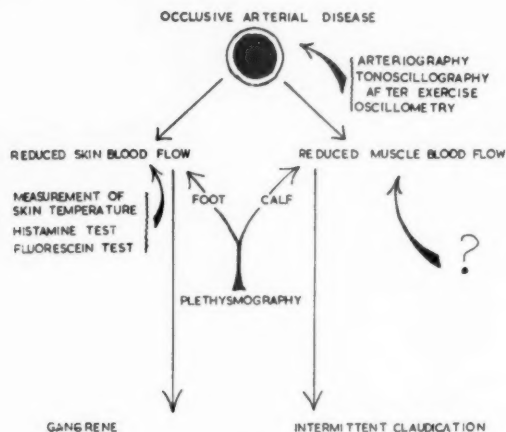
FIG. 1.

great value for research purposes. No satisfactory method has been found for the easy measurement of muscle blood-flow clinically.

TONOSCILLOGRAPHY AFTER EXERCISE

This test has proved to be of great value diagnostically. Although instrumental in the first instance, it has helped one to gain more from the purely clinical examination of the patient. Firstly the instrumental method will be described, and then its clinical application.

In this examination graphic recordings are made of pulsations in a limb before and after exercise. The apparatus consists of an automatic oscillograph, and was designed and its use described by Borje Ejrup of Stockholm (Ejrup, 1948).

Before examination the subject rests quietly on a couch for 15 minutes; a cuff is then placed around the ankle. The cuff consists of an inflatable rubber ring surrounded by a firm cloth covering; it is automatically inflated and deflated. Pulsations in it during the inflation phase are transmitted by means of a piezo-electric manometer and an amplifier to a pen. As the pressure increases, the pen advances vertically and pulsations are recorded in the horizontal plane. The recording paper is marked with horizontal lines, and the paper calibrated so that these mark the pressure increase in the cuff in steps of 50 mm. of mercury from 0 to 300 mm. A tracing is automatically obtained every thirty seconds.

After satisfactory recordings at rest the subject exercises—either by vigorous dorsiplantar flexion of the foot for about 90 seconds while lying horizontal, or by running gently or walking briskly for up to 200 yards.

It is found that in normal subjects there is an increase in the amplitude of pulsations after exercise (normal response), whereas in those with occlusive arterial disease (either complete or incomplete obstruction) the amplitude of pulsations diminishes (inverse response). This lasts for at least two tracings after exercise and may persist for as long as 30 minutes. Subsequently readings are similarly obtained from the calf and thigh, and sometimes the foot.

There is good correlation between the site of arterial obstruction as demonstrated by arteriography, and the level of the inverse reaction. The inverse reaction occurs distal to gross narrowing or obstruc-tion of a major artery and persists distal to that site—thus the upper limit only of the obstruction is indicated. In patients with femoral arterial obstruction inverse reactions are found at the calf and ankle, and in those with popliteal arterial obstruction at the ankle. When the tibial vessels are obstructed an inverse reaction is found at the foot only. Finally, where the aorta or iliac arteries are obstructed, an inverse reaction occurs at the thigh and distally (McDonald and Semple, 1952).

It should be emphasised:

  (1) that tonoscillography after exercise indicates only the upper limit of an arterial occlusion.

  (2) that the information regarding the site of the obstruction refers to the majority of cases; exceptions may occur.

  (3) we have found no case of intermittent claudication in which an inverse response was not obtained.

As regards the last it is interesting to note that inverse reactions after exercise are found in the legs of patients with severe anæmia and coarctation of the aorta; after such conditions are corrected the response on tonoscillography after exercise returns to normal.

CLINICAL TEST

To carry out this the dorsalis pedis and posterior tibial pulses are palpated after a period of rest and their position marked with a skin pencil. The patient then takes exercise as is described above. In the great majority of patients with arterial obstruction the pulses will be impalpable after exercise, or they will have become grossly diminished. This lasts for $\frac{1}{3}$–3 minutes. The clinical test is naturally not as sensitive as the instrumental method but is sufficiently so for the large majority of cases.

Although André-Thomas and Levy-Valensi described this clinical phenomenon in the French literature in 1918 its value has not been generally appreciated.

In conclusion, this test, the examination of the pulses before and after exercise is another example of how instrumental methods may be used to enable the physician to learn more from the purely clinical examination of a patient.

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The Problem of Therapy with Fibrinolytic Substances [*Abstract*]

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A NUMBER of interesting problems are raised by the use of the plasminogen plasmin system and its activator streptokinase in the therapy of human disease. Streptokinase is an extracellular metabolic product of hæmolytic streptococci which is now available commercially in a state of considerable potency.

Numerous enthusiastic clinical reports have been published concerning the use of streptokinase in the removal of deleterious exudate or fibrin from the pleural space, cerebrospinal spaces, and superficial areas of ulceration due to trauma or infection.

The advantage of being able to remove such fibrinous exudate is of course considerable, but it is now apparent that this is at times more difficult than earlier reports would lead one to believe. There is a serious lack of information concerning optimal dosage and the likely results of such enzyme therapy, when applied to the different systems of the body. Dosage has hitherto been on an empirical basis and has had to be judged on individual clinical response.

TABLE I

	Streptokinase antibodies	
	⟵	
	Streptokinase	
	⟶	
PLASMINOGEN	⟶	PLASMIN
(Serum fraction) (Enzyme precursor)	Other activators	(Enzyme)
Plasmin	⟶	1. Lyses fibrin
	⟵	2. Attacks fibrinogen, etc.
	Antiplasmin	
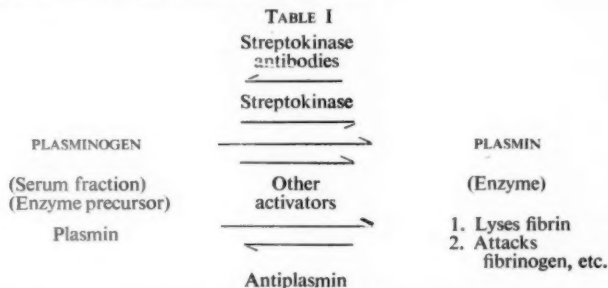
The word fibrinolysin is not employed since previously it has been used to indicate both the bacterial activator of the system (streptokinase) and the active enzyme (plasmin).

The work of Milestone (1941) and later Christensen and MacLeod (1945) has shown conclusively that streptokinase has no fibrinolytic properties and requires a serum factor for its substrate, this is named plasminogen and behaves chemically as a euglobulin. On treatment with streptokinase or other activator, plasminogen acquires fibrinolytic properties and is named plasmin.

Commercial streptokinase preparations contain, to a greater or lesser extent, streptococcal desoxy-ribonuclease or streptodornase and this component is of considerable therapeutic importance but plays no part in fibrinolysis.

Therapy with streptokinase-activated plasminogen is affected firstly by the presence of antibodies to streptokinase and secondly by the presence of plasmin inhibitors. Antibodies with the power to neutralize streptokinase may exist in serum and are increased following the injection of streptokinase or after hæmolytic streptococcal infection. Powerful inhibitors of plasmin exist in normal and pathological body fluids.

The laboratory control of fibrinolytic therapy using this system presents a number of difficulties. Methods for the assay of enzyme precursor, active enzyme, antiplasmin, and antistreptokinase antibodies have to be developed, and the results of these assays interpreted against the background of the rather complex interaction of this system. Final proof as to the accuracy of these calculations must be provided by the demonstration of free fibrinolytic enzyme *in vivo*.

Data illustrating these points, particularly with regard to the therapy of tuberculous meningitis, was shown, and attention was drawn to the possibility inherent in the use of purified plasminogen and plasmin as therapeutic agents.

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Cranial Arteritis

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CRANIAL arteritis is still regarded as uncommon. The fact, however, that in no more than eight years I have had the opportunity of studying some 30 examples of this condition, leads me to suspect that the supposed rarity of the disease is due rather to a lack of appreciation of the picture which it presents, than to any lack of material.

When I first met the illness in 1945, the world literature contained references to only 20 cases, and of these, only 6 were British.

With the increasing interest in diseases affecting the elderly, it seems certain that many examples of the condition will be met, and unless a clear picture is available to the physician, almost certainly the illness will continue to be overlooked.

The history of this disease is of some interest. Horton, Maygath and Brown (1934), reported a condition in which an arteritis involving especially the temporal vessels, appeared to affect elderly patients. Two years previously the same authors had referred briefly to these cases, in the *Proceedings of Staff Meetings of the Mayo Clinic* (1932).

There is no doubt that to these men we owe the interest which has subsequently developed in the disease. It is true, however, that Schmidt in a communication on Intra-cranial Aneurysms, published in *Brain* in 1930, refers to a man, who from the description clearly suffered from this disease. Still more interesting is the reference in a paper by Jonathan Hutchinson, written in 1889 on peripheral vascular disease, to a man seen by the writer some years previously. The precisely worded description of this old man's illness can hardly be improved upon now, and would serve as an excellent clinical account of the disease.

In 1938 Jennings wrote the first account of temporal arteritis in the British literature. Ten years later (1948) in a second important paper, he gave us his further experience of the disease. Since Jennings' first paper, a number of communications have appeared, giving excellent accounts of the clinical and pathological features, perhaps the most important of these in this country being papers by Cooke and his colleagues in Birmingham (1946), Gilmour (1941), and Harrison (1948).

That it can be easily mistaken at autopsy is suggested by a paper in 1935 by Barnard, who described what histologically appeared to represent an example of tuberculous arteritis, in a woman who died of an illness clinically indistinguishable from cranial arteritis.

The illness is almost certainly confined to the latter part of life and relatively few cases have been described before the age of 60. It probably affects men as often as women though thus far a tendency to predominate in women has been suggested. The disease, though severe and liable to produce impressive disability, is self-limited and there is no doubt that its natural history tends towards recovery. It is a long illness often extending over a year or eighteen months. Its greatest threat to life is seen in those cases where serious mental deterioration develops, cases spoken of by Cooke as encephalopathic in type. In the majority of cases where recovery eventuates, it presents a constant threat to vision, and at any time in its course total blindness in one or both eyes may result from involvement of the vessels supplying the optic nerve. On the whole, however, where sight is involved, this complication tends to appear relatively early in the illness.

A typical example of the disease presents a very striking and I believe characteristic picture, offering little diagnostic difficulty to anyone aware of the condition.

An elderly person, previously in good health, and with a non-contributory past history, becomes ill. The onset is subacute, and the early symptoms include loss of energy with failure of appetite, depression, headache, rheumatism, fever, sweating and invariably weight loss. Any one of these features may predominate, though headache, by far the most constant symptom, may not be present from the onset. When it does develop it is severe, constant, and reacts indifferently to analgesics. As the headache is really indicative of tenderness of the scalp, even light pressure is intolerable, and sleep becomes almost impossible.

Another almost constant symptom is that of pain during mastication. The rheumatism is severe, affects muscles and rarely joints, and again responds badly to analgesics. Fever, usually of low grade, may on occasion be impressive—continues for many weeks, occasionally showing spontaneous remissions. Sweating is often severe enough to suggest a brucellosis and weight loss may be severe enough to raise the possibility of tubercle, thyrotoxicosis, diabetes and especially malignant disease. It is this last possibility which is often so worrying for the picture presented often suggests malignant cachexia. In a few cases transient neurological lesions appear, and in some, symptoms suggesting gastric ulcer present.

Clinical examination is disappointingly negative, and even the tender, swollen cranial arteries may be overlooked. When these are noted, pulsation may still be present, and this should not mislead. In the few who show involvement of the adjacent scalp tissues in a subacute cellulitis, the diagnosis must suggest itself. Lesions of the cardiovascular system are not a characteristic finding, apart from the cranial artery involvement, and hypertension, if present, precedes the development of the disease. In some, the lymph nodes in the neck are enlarged and tender, but enlargement of the liver and spleen, if present, is due to other pre-existing causes.

Almost invariably a secondary anæmia develops and the white cell count rises. The E.S.R. is usually accelerated, and a normal finding here makes the diagnosis improbable. The W.R. is invariably negative in both blood and C.S.F., but the latter may show an increase in cells and protein. In a number there is some reduction in the total blood proteins, with alteration in the A/G ratio. Renal function is not impaired, and changes either in the urine, blood urea, or urea clearance are due to factors other than the disease.

Perhaps the most suggestive feature of the whole picture is the patient's appearance. Typically the face is lined and expressionless, with evidence of weight loss, eyes lack-lustre, and an expression indicative of depression and loss of emotional tone. Little is said, and in taking the history, leading questions early become necessary, and to these the patient tends to reply in toneless monosyllables. Not infrequently, the physician acquainted with the disease may suspect the condition at first sight.

Where vision is impaired or lost, fundal changes are surprisingly insignificant. In a few blind eyes, evidence of occlusion of the central retinal artery or its branches may be present, though more usually there is no more than a pale, clear-cut disc, the nutrient artery of the nerve, behind the orbit, having been affected. In a few cases, seen soon after loss of sight, one may observe fragmentation of the venous blood flow.

Treatment is unsatisfactory. Section of affected cranial arteries almost invariably results in a reduction in local pain and headache, while local infiltration of the same vessels with anæsthetic has had the same result. Salicylates in full doses appear to be symptomatically more effective than any other form of analgesia.

The only antibiotic which has promised any success has been Aureomycin, and a recent paper from Oxford (Rice-Oxley and Cooke, 1951) reports success with this drug. I have used Aureomycin in 4 cases, in only one of which did it appear to be effective.

The anæmia responds to nothing but blood transfusion, and several cases thus treated appear to have had the whole course of the disease modified and remission accelerated.

I have had the opportunity of treating six proved cases with cortisone and ACTH. In each a dramatic and immediate improvement became apparent. This was maintained while the treatment was continued, but on stopping the treatment symptoms gradually recurred, though in no case was the relapse anything like as immediate as, for instance, in rheumatoid arthritis. My general impression has been that where it has been possible to use either cortisone or ACTH, the natural history of the disease may possibly have been altered and shortened.

In all, I have seen 28 cases during the seven years since 1945. Of these, 14 have had characteristic histological appearances in biopsy material. Of the remaining 14 cases in which biopsy was not performed for one reason or another, 12 had shown the typical changes in the cranial arteries at some time in the disease.

The histological changes manifest themselves as a panarteritis of somewhat subacute or granulomatous character. The intima is invariably much thickened, though the lumen of the vessel is rarely occluded by thrombus. There is fragmentation of the internal elastic lamina, while the media is involved in an exudative rather than necrotizing process. Giant cells, of the foreign body type, are common, and are usually seen in the outermost layers of the media. Eosinophils are relatively uncommon. The adventitia is involved in the exudative process, and adjacent structures may on occasion be implicated.

As compared with polyarteritis nodosa, the following major differences at once become apparent:

Cranial arteritis occurs after the age of 55, and though polyarteritis is by no means confined to young people, it is certainly more common in the earlier age groups, and may affect young children. Cranial arteritis is intrinsically benign, and there is no doubt that it tends towards spontaneous recovery. When recovery does occur, unless sight has been affected, it is complete. In polyarteritis the disease is in every case potentially lethal, and when recovery occurs, some permanent damage almost invariably remains in its wake.

Renal, pulmonary and intestinal involvement are almost unknown in cranial arteritis, whereas they are common and serious in polyarteritis. Histologically, not only are different arteries involved, but in polyarteritis the arteritis is more acute, necrotizing in type and frequently resulting in aneurysm formation.

Of all the names suggested for this disease cranial arteritis appears to be the most satisfactory, though there is no longer any doubt that the disease is not confined to these vessels.

In my series, most of the cases presented in the characteristic manner, and there have been few unusual features. One man, however, developed the murmur of aortic incompetence three months after the appearance of his illness. Two had gastric ulcer, and in two others occult blood was constantly present in the stool. Two developed thyrotoxicosis during convalescence, and in one a lymphocytic meningitis appeared for a short while, early in the disease.

Sight has been less frequently damaged in my series than others have noticed and there have been only eight blind eyes, with almost sixty at risk. In all these the blindness has been permanent.

As to the outcome of the illness, only 5 of my patients have died, and in only 2 of these could death be attributed to the illness in question. One died from a perforation of a gastric ulcer, and the second from a cerebral catastrophe, both these events occurring during the height of the illness. The other 3 patients died from cardiovascular accidents, long after their arteritis had resolved.

I have not seen any case which could be designated as being of the encephalopathic type referred to by Cooke, and in which type the outlook is grave.

Little has been learnt about causative and predisposing factors. In spite of the features suggesting an infection, no evidence for such has come to light, and the failure to respond to antibacterial treatment makes it improbable that infection plays any direct part. I have noticed a previous history of rheumatism, in a few patients. In several there has been trauma either accidental or surgical, immediately before the appearance of symptoms, and in a number there has been emotional stress of sufficient severity to suggest a possible relationship.

The general behaviour of the illness, the histopathology, and the response to cortisone and ACTH, all suggest that the illness is probably related to the other collagen diseases.

Recently I have studied a number of cases in which there has been gross anterior pituitary damage, in two as a result of chromophobe tumour. In these people, a syndrome reminiscent of cranial arteritis, apart from actual cranial artery involvement, has developed and has responded well to ACTH and cortisone. These patients all suffered not only from all the symptoms and signs of hypopituitary function, but also complained bitterly of a rheumatism very similar to that met with in cranial arteritis. In one, biopsy of a subcutaneous nodule showed an arteriolitis characteristic of Erythema Induratum—and in two others hyaline changes in voluntary muscle fibres were found.

It seems possible that a senile hypopituitary state may underlie this disease, with stress in the form of trauma, emotional disturbance or infection, acting as a precipitating factor. There seems to be no evidence that allergy in the usually accepted sense of the word, or drug sensitivity, plays any part in the development of the syndrome.

CONCLUSION.

Cranial arteritis is probably a common disease. I would urge all physicians, ophthalmologists, rheumatologists and geriatricians to add it to their diagnostic vocabulary. The illness itself may be so severe that the presence of some occult but lethal condition may be suspected.

The completeness of recovery in these cases is most impressive, and after an illness of such severity and length, in which a grave or even hopeless outcome has been suggested, it is professionally unsettling when the moribund rise to full life and vigour once more.

I would suggest that this diagnosis be considered in all elderly patients complaining of unexplained headache, fever, loss of weight, rheumatism and general ill-health.

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Charcot Joints in Diabetes Mellitus

By M. MENCER MARTIN, M.D., M.R.C.P.

King's College Hospital

LESIONS of the feet in patients with diabetes mellitus are generally ascribed to occlusive vascular disease or sepsis and the importance of peripheral nerve disease as a causal factor is insufficiently recognized. Painless neuropathic ulcers and Charcot joints, although often stated to be common only in tabes dorsalis and syringomyelia, are by no means uncommon in diabetes.

Yet it was only recently (Jordan, 1936) that Charcot joints were recognized in connexion with diabetic peripheral nerve disease and as late as 1951 Parsons and Norton claimed that fewer than 22 cases of diabetic arthropathy had been recorded in the world literature.

In the course of a recent investigation at King's College Hospital 12 cases were discovered with bone and joint lesions closely resembling•Charcot joints, which leads one to suspect that diabetic arthropathies may be more common than the rarity of reports would suggest.

Cases.—The age and sex incidence of the present cases are shown in Table I. In 4 cases there was

TABLE I.—THE AGE AND SEX DISTRIBUTION OF THE 12
PATIENTS WITH DIABETIC NEUROPATHIC JOINT LESIONS

Age group	Sex		Total
	M.	F.	
20–30			
30–40	2	1	3
40–50	3		3
50–60	1	1	2
60–70	1	3	4
Total	7	5	12

involvement of the interphalangeal or metatarsophalangeal joints, in one of them of both feet (Fig. 1). In 3 cases radiographs of the feet showed attenuation of the metatarsal shafts which tapered distally, absorption of the metatarsal heads and deformities of the foreparts of the feet (Fig. 2). In 5 cases the midtarsal joints were affected giving rise in each case to a deformity consisting of eversion and external rotation of the forepart of the foot and flattening of the longitudinal arches (Fig. 3). The radiological appearances were those of a hypertrophic arthropathy with considerable destruction of

FIG. 1.—The proximal interphalangeal joint of the left 4th toe and the right 3rd toe shows an arthropathy with disorganization of the joint, absorption of subarticular bone, but without any evidence of infection or bone resorption.

FIG. 2.—Radiograph showing attenuation of the shaft of the left 5th metatarsal bone with absorption of the head and resulting deformity of the little toe.

subarticular bone and excessive new bone formation in the surrounding soft tissues but without periosteal reaction or evidence of bone absorption (Fig. 4).

In the patients with lesions of the interphalangeal or metatarsophalangeal joints the arthropathies were often discovered only after X-ray examination. In the cases with arthropathies of the midtarsal articulation attention was usually drawn to the condition by the presence of deformity.

FIG. 3.—Photograph of the right foot of a patient with diabetic neuropathy and arthropathy. The prominence of the navicular bone with thickening in the midtarsal region and eversion and lateral rotation of the forepart of the foot are well shown.

FIG. 4.—Radiograph of the right foot showing deformity of the tarsus with proximal displacement of the lateral metatarsals. The medial three tarsometatarsal joints show a severe arthropathy with considerable destruction of subarticular bone. There is no evidence of osteoporosis.

In most of the cases, as in those reported in the literature, the lesion was of slow onset and gradual progression over a period of some years. However, in one case the arthropathy developed rapidly with sudden swelling of the foot, local redness and a temperature in the absence of infection as occasionally occurs in the classical arthropathy of Charcot (Wilson, 1940). The gross disorganization of the joint with œdema and local heat raised the possibility of an infective condition being, at least in part, responsible for the extensive bone destruction. However, the erythrocyte sedimentation rate was normal and joint fluid obtained by aspiration proved sterile both on direct examination and on culture. As the patient had at no time been given antibiotic or chemotherapy it was felt that the above laboratory findings were strong evidence against an infective arthritis which, in any case, was not supported by the radiological appearances. A similar case has recently been reported by Lister and Maudsley (1951).

Evidence of the presence of occlusive peripheral vascular disease was found in only one of the 12 cases. In fact most patients had strong pedal pulses and vascular studies with the Boulitte oscillometer and skin-temperature measurements following the intravenous injection of 50 mg. Priscol failed to show impairment in the peripheral circulation.

Soft-tissue lesions of the feet were present in 6 patients at the time the arthropathy was first diagnosed, but were generally neither in close proximity to the involved joint nor associated with deep-seated infection. 5 patients gave a history of perforating ulcers or septic lesions about their feet, which had, however, preceded the development of the joint condition often by as much as five years. In 3 of these cases the affected joints were known to have been normal radiologically at the time the patients were treated for their ulceration. In 2 cases joint changes were discovered in both feet, although skin lesions were or had been present in only one foot.

Pathological evidence of the presence of nerve disease was obtained by means of nerve biopsies in 4 cases with bone lesions included in the present series. The histological sections showed varying degrees of myelin degeneration and of axis-cylinder degeneration without, however, any sign of endo- or peri-neural fibrosis or degenerative disease of the vasa nervorum.

DISCUSSION

Bailey and Root (1942) collected 14 cases with degenerative bone lesions in diabetics and stated that the condition was not dependent on deficient blood supply. In 1947 the same authors reported painless destruction of the tarsus in 17 cases with chronic poorly controlled diabetes and thought that the bone destruction was the result of diabetic neuropathy and similar to the arthropathies of neuro-syphilis, syringomyelia, nerve injuries and the neural form of leprosy.

In all cases in whom arthropathies were discovered in the present study, there was good evidence of neglected diabetic control and of the presence of a nerve disorder. The most characteristic feature was the absence of pain throughout the development of the joint changes. Diabetic neuropathy affects mainly the lower limbs at their periphery and with distal involvement of the nerves it was to be expected that the joint changes would be most common in the peripheral joints. A diabetic neuropathic arthropathy of the knee has been reported (De Takats, 1945; Spear, 1947; Shore, 1947), but appears to be very rare. Involvement of the ankle-joint seems to be more common (Foster and Bassett, 1947; Muri, 1949; Knuttson, 1951; and others) but neither was met with in the present series.

As regards the pathogenesis of the neuropathic joint lesions various theories have been proposed but most of them are open to criticism.

The frequent association of perforating ulcers and neuropathic joints has often led to sepsis being thought responsible for the bone changes. Hodgson, Pugh and Young (1948) expressed the opinion that infection leading to osteomyelitis was the direct cause of the bone change independent of any associated nervous disorder. The denial that a disturbance of nerve supply was responsible for the bone as well as the soft-tissue lesions does not seem well-founded particularly as there is abundant evidence that damage to the nervous system can and does produce such lesions. The soft tissue and bone lesions occur independently and are undoubtedly secondary to the peripheral nerve disorder of diabetes mellitus with loss of pain sensibility.

Foster and Bassett (1947) thought that they had obtained support for Charcot's original theory (Charcot, 1868) that neuropathic joints resulted from lesions of the autonomic nerves. They carried out investigations into the functional integrity of the autonomic nervous system in their two cases and found impairment or loss of sympathetic activity in both of them. They quoted Dreyfus and Zacharovitch (1937) and Wartemberg (1938) as having produced evidence of autonomic nerve damage in the cases of neuropathic joint lesions of syringomyelia and tabes dorsalis, and expressed the opinion that the association of loss of proprioceptive perception and autonomic nerve dysfunction predisposed to the development of a Charcot joint.

In the course of an investigation into diabetic neuropathy (Martin, 1952) evidence of autonomic dysfunction was discovered in all the cases of diabetic nerve disease with or without joint changes, which leaves little support for the hypothesis that such nerve damage *per se* predisposes to the development of the arthropathy. Furthermore evidence has been obtained that autonomic nerve disturbance is not essential to the development of a Charcot joint. Tests of vasomotor function in cases of tabes dorsalis have shown that joint lesions may be present in this condition with intact vasomotor nerves.

Eloesser (1917) produced what appeared to be Charcot joints experimentally in animals by subjecting joints to operative trauma after they had previously been rendered anæsthetic by resection of the appropriate posterior nerve roots. McMurray (1950) published the report of a case who from birth had shown marked insensitivity to pain, in whom a deformity was present of the left ankle suggesting a Charcot joint.

Neither autonomic nerve damage, ischæmia, nor sepsis can explain adequately the pathogenesis of a neuropathic arthropathy. Diabetic neuropathy shows a predilection for non-myelinated nerve fibres (Martin, 1952) which causes pain fibres to be involved early. It appears that the "mechanical theory", postulating that loss of afferent impulses allows minor trauma to damage the joints, has most to recommend it. The presence of histological evidence of nerve degeneration in all the 4 cases with diabetic arthropathy in whom nerve biopsies were obtained and the absence of Charcot joints in patients without diabetic neuropathy in the diabetic population attending King's College Hospital support the belief, that these bone changes are truly neuropathic and develop only in the presence of nerve disease and continued weightbearing.

Whilst the prognosis for the neuropathic soft-tissue lesion is usually good provided it is recognized early and treated, the prognosis of the neuropathic arthropathy appears to be bad and none of the cases has shown any tendency to improve. However, not all cases have continued to advance, in fact most of them have become arrested with no demonstrable change in the radiological appearances over the past few years.

Recently Parsons and Norton (1951) suggested that sympathectomy may be of benefit in treatment.

However, in view of the fact that the sympathetic fibres are already damaged in cases of diabetic neuropathy (Martin, 1952) it is difficult to see why surgical sympathectomy should prevent progress of the bony lesions which in any case tend to become arrested with treatment of the neuropathy.

As suggested by Marble (1948) the only treatment which proved helpful in progressive cases was the use of orthopædic appliances which prevented weightbearing and further deformity. Successful treatment depends on early diagnosis before gross deformity has developed and for that reason all cases of diabetic neuropathy and particularly those with soft tissue lesions should have their feet X-rayed routinely as otherwise only the late examples come to light and the early ones which are the most amenable to treatment will be missed.

Both neuropathic skin and joint lesions are the result of the underlying nerve disorder consequent upon poor diabetic control, and it is therefore most important that attention be directed towards treatment. Unfortunately there is, as yet, no specific cure for this complication of diabetes mellitus beyond rigid control of the metabolic disorder. With adequate diabetic treatment, however, recovery may be expected in about 60% of cases within six months to two years, unless the neuropathy has been of long standing. With recovery of the nerve disorder the progress of the bone changes appears to be halted.

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Section of Neurology

President—Professor P. C. P. CLOAKE, M.D., F.R.C.P.

[*February* 7, 1952]

MEETING AT THE MAIDA VALE HOSPITAL FOR NERVOUS DISEASES, LONDON, W.9

Unilateral Pulsating Exophthalmos Associated with Multiple Bony Tumours (Some Pulsating) and a Nodular Thyroid Swelling.—DIANA BECK, F.R.C.S.

C. D., a storekeeper, aged 58.

August 1951: Prominence of patient's left eye noticed by his daughter. Two to three months' watering of left eye and diplopia on looking to the left. Bronchitis since 1914–1918 war, with productive morning and evening cough for years. Stiffness and pain in left hip for three to four years.

On examination.—Healthy-looking man. B.P. 130/80. Lobulated swelling, 3½ in. × 2½ in. behind left sternomastoid muscle and extending into thoracic inlet. Separate rounded nodule, 1 in. diameter, in mid-line over trachea. Both move on swallowing. Tracheal deviation to the right. No fixity to or displacement of carotid sheath. (Patient says swelling present for four years without change in size.) Sebaceous cyst in scalp. Small left hydrocele. Lipoma in front of middle of left thigh. Equinovarus deformity of right foot.

Left eyeball displaced forwards and downwards (Fig. 1) pulsates synchronously with the heart. Bruit over globe and anterior part of left temporal fossa: both pulsation and bruit disappear on
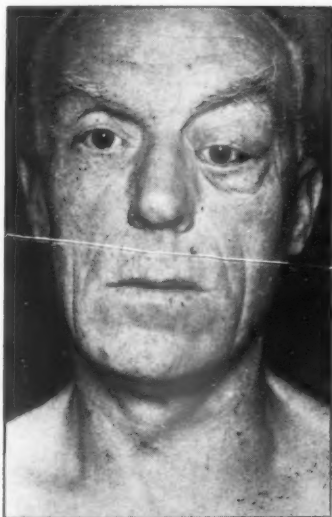
FIG. 1.—Photograph showing forward and downward displacement of left eyeball and nodular thyroid swelling.
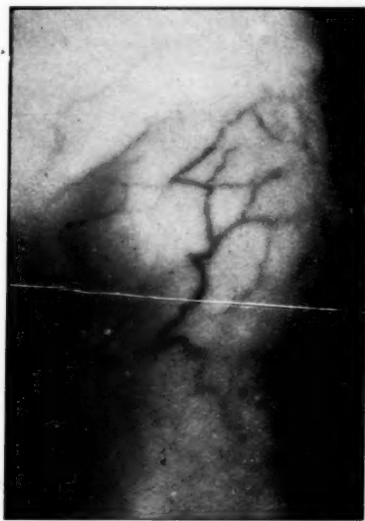
FIG. 2.—Swelling in left iliac and upper femoral region (pulsating). Lipoma in mid-thigh. (Infra-red photograph.)

carotid compression and the proptosis diminishes. V.A.R., J 1. V.A.L., J 10 (both corrected). Left papilloedema. Diminished outward and upward movement of the left eye with diplopia on looking in these directions. Proptosis 12 mm.

Large, firm, pulsating swelling in region of left hip, involving lateral aspect of ilium, outer half of left iliac fossa and upper and lateral aspect of thigh (Fig. 2). Mass fixed to deep structures but not to skin. Pulsation and bruit heard over mass, synchronous with the heart. Internal rotation of left hip-joint limited. Wasting of right thigh and leg (associated with congenital deformity of right foot).

Enlarged prostate; no fixity.

Investigations.—X-ray skull: Irregularity and thickening of medial third of left lesser wing and diffuse thickening of both greater and lesser wings of sphenoid with obliteration of linear shadow forming anterolateral wall of middle fossa as seen in P.A. views. Thought to indicate diffuse infiltration by tumour, meningioma being most likely.

X-ray chest: Opacity in right 6th rib in axillary line around an area of destruction.

AUG.—NEUROL. 1

FIG. 3. — Left carotid arteriogram showing diffuse " blush " of greater and lesser wings of sphenoid in capillary and venous phases.
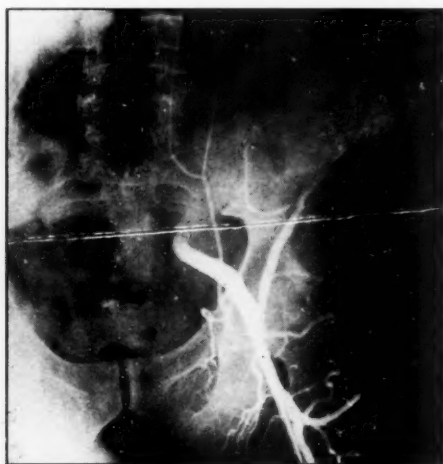
Left carotid arteriogram showed a diffuse "blush" of contrast in the capillary and venous phases involving most of the greater wing of sphenoid and lesser wing as far medially as anterior clinoid. The appearances suggested a meningioma but in view of the chest X-ray, deposit could not be excluded (Fig. 3).

X-ray pelvis: Large area of erosion in left ilium directly above the acetabulum. There is slight irregular sclerosis around the defect and irregular bony trabeculation extending through it, with extension into the surrounding soft tissue. There is another area of erosion in the right side of the sacrum with slight surrounding bony sclerosis.

X-ray pelvis, previously elsewhere; tumour of ilium.

Retrograde left femoral arteriogram: Hypertrophied external iliac artery and the superficial circumflex iliac artery is grossly hypertrophied as it passes upwards to supply the tumour. Other abnormal vessels arise from the lateral femoral circumflex and profunda femoris arteries to supply the tumour which is extremely vascular (Fig. 4).

The capillary and venous phases show an enormous blush of pathological vessels throughout the lesion (Fig. 5).

FIG. 4.—Retrograde left femoral arteriogram.

FIG. 5.—X-ray of pelvis showing enormous " blush " in capillary and venous phases of left femoral arteriogram.

E.S.R. (Wintrobe) 28 mm. in 1 hr. Sputum: no malignant cells. I.V.P. raises possibility of right renal tumour. Large prostatic impression in bladder. Radio-active iodine test inconclusive. B.M.R. + 17. Biopsy of iliac tumour: secondary carcinoma. Serum calcium: 10·8 mg.%. Alkaline phosphatase 5·5 K.A. units.

Comment.—A unilateral pulsating exophthalmos developed gradually in this patient and is associated with a bruit much less obvious than that heard with a carotico-cavernous aneurysm.

At first, this patient was thought to have a meningioma of the greater and lesser wings of the left sphenoid to which the findings in the straight X-rays and an arteriography subscribed. Of Cushing's 53 cases of meningioma of the sphenoidal ridge only two had a bruit: this was heard in the temporal region in cases of meningioma *en plaque* and in neither was there pulsation of the eye.

FIG. 6.—Thyroid removed for carcinoma.

The diagnostic significance of pulsation in bony tumours has often been questioned. The clinical findings of a tumour of the thyroid, apparently benign, and a large pulsating tumour of the ilium in addition to the pulsating exophthalmos made it almost certain that the presenting symptom was due to metastasis in the orbit.

POSTSCRIPT.—Since this case was demonstrated, Mr. Vaughan Hudson has performed a total thyroidectomy for carcinoma of the thyroid (Fig. 6). Radioactive iodine had no effect in staying the progress of the disease and the patient is now undergoing deep X-ray therapy to the metastases. At the time of going to press the exophthalmos has diminished but there is a marked impairment of visual acuity of the left eye.

Sarcoidosis with Involvement of Central Nervous System.—ALEXANDER KAHAN, M.D., M.R.C.P.E.

S. F., a cinema projectionist, aged 30, came to hospital in February 1951 complaining of numbness in the finger tips, which had started about July 1950. Intermittent at first, it had become continuous. At times he had pins and needles in his fingers and at the back of his neck. In the morning his fingers were stiff and his grip weak. His hands were clumsy and he had difficulty in dressing and in doing his work. There was some numbness in the right thigh. He had no ocular symptoms and no sphincter disturbance. His previous health had been good. During the war he had served in the East and his only illness had been a "desert sore". He was tattooed in 1940 and in December 1950 he first noticed some nodules in the tattoo marks.

He was a well-built man with no abnormal physical signs in cardiovascular system, lungs or abdomen. B.P. 140/95. Blood W.R. negative. There was blunting to pin-prick on the tips of the fingers, and a doubtful left plantar response: no other abnormal signs were found in the nervous system. An X-ray showed a bilateral cervical rib. In March 1951 he was admitted to hospital for further observation. Sensory symptoms were unchanged but an increase in neurological signs occurred. The left plantar became extensor and later the response on the right was also abnormal, and a left ankle clonus developed. The left upper abdominal reflex could not be obtained. Vibration sense in his arms was impaired. A left lower facial weakness appeared. Lumbar puncture was entirely normal, including the Lange. Blood count and fractional test meal were both normal.

In the tattoo marks over each deltoid and on the right forearm small round nodules, about 4 mm. in diameter, were observed. They were quite painless. During April 1951 axillary adenitis developed. Biopsies of the skin nodule and of an axillary gland were carried out and both showed appearances typical of sarcoidosis. X-ray of the chest (previously normal) was repeated and showed slight but definite changes in the lung fields. The hands and feet were radiologically normal.

The neurological condition consisted of a left facial weakness, left pyramidal signs suggesting a lesion in hemisphere or brain-stem, and sensory changes in the arms suggesting a cervical cord lesion. Since April 1951 there has been little change in the neurological state, except that at times his speech has been rather slurred. There must be multiple lesions within the central nervous system, and in view of the proven diagnosis of sarcoidosis involving the skin, lymph glands and lungs, it is suggested that the C.N.S. is similarly involved with multiple sarcoid deposits. (Sections from skin and lymph gland were demonstrated, with typical non-caseating collections of endothelial cells and lymphocytes forming false follicles, together with giant cells with horseshoe nuclei.)

Comment.—Many cases in which the C.N.S. is involved in sarcoidosis have been published. Colover (1948) analysed 115 from the literature and added three more, and Essellier *et al.* (1951) in a shorter review of the subject, include 9 not quoted by Colover. Denis Williams (1950) presented a case to this Section. Essellier and his colleagues divide the cases into meningitic, meningo-encephalitic, and encephalitic forms, and they stress the variability of the symptoms and of the course of the disease. Both they and Colover describe many cases as presenting with a polyneuritis. Facial palsy seems especially common. Sometimes it follows involvement of parotid but may be the initial symptom (Freiman, 1948).

There have been favourable reports of treatment of sarcoidosis with massive doses of calciferol, and for this reason this patient was given 100,000 units daily, from June to October. During this time the skin lesions slowly resolved but no effect on the neurological condition was apparent.

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Cerebellar Degeneration Associated with Carcinoma of the Lung.—R. A. HENSON, M.D., M.R.C.P. (for Sir RUSSELL BRAIN, D.M., P.R.C.P.) This case will be published elsewhere.

Von Hippel-Lindau Disease affecting the Spinal Cord.—S. NEVIN, M.D., F.R.C.P., and G. MONCKTON, M.D., M.R.C.P.

N. M., female, aged 38. 1936: At the age of 23 noticed a patch of numbness under the left breast. 1938: Experienced severe pain in the left chest when riding, passing off after an hour or so. 1942: Following miscarriage, developed severe pain in the left chest, shoulder and thigh, which has persisted with varying severity ever since.

1945: Male infant born. Following this her pain became very much more severe, worse on movement. Began to have urgency of micturition and occasional incontinence of urine.

On examination.—Cranial nerves were normal; fundi and discs normal. Power and tone normal in the limbs. There was an area of analgesia and anæsthesia extending from the lower half of the breast to the fold of the groin anteriorly on the left side. There was also a band 2 in. wide of analgesia and anæsthesia at the level of dorsal segment 12 on the back. Below these levels all sensory modalities, apart from vibration sense, were normal; the latter was equally diminished at the ankles. Reflexes were symmetrical and normal, plantar responses flexor.

Investigations.—Cisternal myelography: suggested a hold-up at level D.10 but no definite block. C.S.F. (from cistern): Protein 230 mg. %, globulin excess.

18.12.45: *Operation.*—Laminectomy at D.10 and 11 by Mr. Wylie McKissock. There was diffuse enlargement of the cord which was not pulsating. A fine needle was inserted into the enlargement and 10–15 c.c. of yellow fluid aspirated. The protein content of this fluid was 2,000 mg. %. The wound was then closed. Following operation there was a temporary amelioration of the pain. In the last few years the pain has gradually increased. Now she is unable to sit up because of the pain and she has 500 mg. of pethidine per day to keep her comfortable. During the last year she has noticed dimness of vision in the right eye.
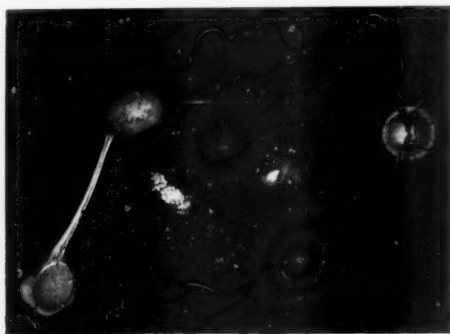
(T. R. Hooper).

FIG. 1.—Optic fundi showing two hæmangioblastomata in the right, and one in the left fundus.

On examination now.—Fundi: The right fundus shows two and the left one hæmangioblastoma (first observed by Miss Kane, 1951) (*See* Fig. 1). The sensory changes noted in 1945 have extended upwards to the level of dorsal segment 2 and down to lumbar segment 1. There are slight pyramidal and posterior column signs in the left leg.

Family history.—Mother and elder sister both died of brain tumours. One cousin suffers from a "spinal complaint".

[*March 6, 1952*]

The following Cases and Specimens were shown:

Precipitation of Symptoms of Arnold-Chiari Malformation by Meningovascular Syphilis.—Dr. D. M. LEIBERMAN and Dr. A. L. WOOLF.

Glomus Jugulare Tumour Causing Multiple Cranial Nerve Palsies.—Dr. K. W. G. HEATHFIELD.

(1) Preparations from Acute Meningo-encephalitis in an Infant Due to an Organism Morphologically Identical with Toxoplasma. (2) Histological Material from Experimental Toxoplasmosis in Animals.—Dr. R. A. GOODBODY.

Sensory Neuropathy in a Case of Carcinoma of the Upper End of the Œsophagus.—Dr. M. C. H. DODGSON and Dr. LOVELL HOFFMAN. For details of this case *see Ann. intern. Med.* In press.

(1) A Deformity of the Roof of the Fourth Ventricle. (2) An Unusual Ependymoma. (3) A Tumour for Diagnosis.—Dr. F. D. BOSANQUET and Dr. P. DANIEL.

(1) On the Mesodermal (Microglial) Origin of the Globoid Cells in Certain Cases of Diffuse Cerebral Sclerosis. (2) Vascular Lesions of the Brain-stem Secondary to Raised Supratentorial Pressure.—Dr. W. BLACKWOOD.

A Case of Neuro-myelo-encephalitis.—Dr. W. G. P. MAIR.

Section of Psychiatry

President—Desmond Curran, F.R.C.P., D.P.M.

[*April 8, 1952*]

Anxiety States and their Treatment by Intravenous Acetylcholine [*Abridged*]

By Professor J. Lopez Ibor (*Madrid*)

The study of some cases of agoraphobia under psychotherapy for a prolonged period gave me the opportunity of observing that they ran a phasic course. I remember particularly the case of a physician whose considerable psychological understanding provided the opportunity for a special study of all his morbid experiences. I saw his agoraphobia increase and diminish like the rise and fall of the tide, basically independent of the outcome of the psychotherapeutic analysis.

Among the various works dedicated to the study of agoraphobia there are two predominant interpretative schools or tendencies. The more classical considers agoraphobia as a manifestation of an obsessive personality, and the problem is one of studying obsessive personalities as variants of psychopathic personalities. Arising out of this are the psychopathological studies which regard obsessions as primarily determined by a disturbance of thought.

The other interpretation springs fundamentally from psychoanalysis. Phobias, like obsessions, are considered to be rooted in repressed complexes, so that external or internal conflicts are manifested literally or symbolically in some definite symptom which is the phobia or obsession. According to this interpretation, the difference between phobias and obsessions lies in the more or less symbolic character of the symptoms unrelated to the determining experiences or complexes.

The observation made of the phasic course of cases of agoraphobia led me to think of the possibility of a middle road between these two interpretations. This middle road would allow the approximation of these cases to other diseases with a phasic course in psychiatry. It is well known for instance that mania and melancholia show an essentially phasic course even though the disease from the clinical standpoint has a constitutional basis in the majority of cases. Later I observed many patients diagnosed as anxiety neurosis or anxiety reaction, whose study, no matter how profound and detailed it was, did not always reveal the existence of a psychogenic mechanism. Nor was their cure brought about by the discovery of an apparent psychogenesis. Further, the supposed anxiety neurosis or anxiety reactions also had a phasic character similar to the cases of agoraphobia mentioned above.

To investigate this relationship I proceeded to:

(1) Group all cases of obsessions, phobias, and anxiety reactions with a phasic character.

(2) The study of the psychological structure common to all these cases.

(3) A search for a biological basis explaining their phasic course, and their evolution independent of the diverse external or internal events in the individual history.

The manic-depressive psychosis offered a good model for these studies. The manic or melancholic phases are classically characterized by the presence of three primary symptoms recognized by Kraepelin. *Mania:* (1) Euphoria, (2) Flight of ideas, (3) Psychomotor excitation. *Melancholia:* (1) Depression or sorrow, (2) Inhibition (retardation) of thought, (3) Psychomotor inhibition.

Subsequent psychopathological analyses showed that in melancholia the retardation of both thought and psychomotor activity is a simple expression of the fundamental experience of the melancholic, which is depression or sorrow. The correlative process occurs in mania. The problem then boiled down to finding out whether the melancholic patient's sorrow has something distinguishing it qualitatively from ordinary normal sorrow. Psychiatrists are divided in their opinions on this point although there is a slowly growing tendency for the majority to admit a qualitative difference between the sorrow of melancholia and the sorrow experienced by a normal person receiving sad news. However, acceptance of this is not general.

Yet it is evident that the sadness of the melancholic patient is *unmotivated*, whereas the sadness of the normal person is produced by an external event. This is so because, from the psychopathological viewpoint, there are two planes of feeling, namely, *reactive or directed feelings*, and *vital feelings*. The former depend upon occurrences or events in the subject's environment which impinge upon him. The latter which we, following Scheler, call *vital feelings*, arise from the subject's organic make-up.

The vital feelings would correspond roughly to what has been called coenesthesia. Every individual is aware of how he feels at a given moment. He is aware of the feeling of freshness, of lassitude, of malaise and of fatigue. In this group of global sensations should be included two others, fundamental from the pathological point of view, namely, sadness and anxiety, which, in order to distinguish them from those caused by external events, should be called *vital sadness* and *vital anxiety*. They are essentially internal experiences arising from a functional disturbance.

It is debatable whether these global experiences correspond to a sum total of the thousands of isolated perceptions that make up coenesthesia, or are another primary experience; that is, they do not come

into our conscious life as the crystallization of an integrative process but as an immediate unrelated experience in itself. The melancholic patient besides being sad, refers to sensations of precordial or abdominal oppression, and at times to diffuse pain in the limbs. This associated series of sensations is more coenesthetic in character than is the sadness itself.

Where there is the clinical picture of vital anxiety (not reactive or psychogenic), I suggest this group should be isolated and called Anxiety Timopathy. This would be one of the various morbid groups of endogenous origin which we shall presently describe. This group includes a majority of so-called anxiety neuroses, many obsessive neuroses and some other cases scattered in various nosologic groupings. The features in common in this group are: similar range of symptoms, the same pathogenic basis and the same response to treatment.

The primary symptom of anxiety timopathy is *vital anxiety*. The patient describes it in various ways. A peasant, for example, comes to my clinic complaining of tremors and rapid breathing, symptoms that become more marked in the presence of the physician. In this case, the physical symptoms which might at first glance be interpreted as psychogenic, are really the expression of a state of anxiety which he does not know how to describe. There are other cases in which anxiety can be present and manifest itself in various symptoms without being known as anxiety by the patient. However, these cases are in the minority. Most patients express a feeling of restlessness, a dread that "something is going to happen". It is the feeling of a vague danger threatening them from they know not where. In describing this, we might say in the language of existentialism that the danger comes from nowhere. In reality it is an internal experience and the feeling of danger arises from the self. The patients may speak of their dread of going mad or of something happening to them in the head. Both forms of expression refer to an internal experience which a psychological analysis always finds represented by the threat that the self might lose control over his acts. Anxiety then is a *fear of disintegration* of the Ego and in consequence there looms the possibility of acting on the instinctive drives, especially the instinct of aggression.

Classification

Clinically, all the diseases which go to form the timopathic group may be classified as follows:

1st. Strictly speaking the manic-depressive psychosis should be set at one end of the series. In it, the most common forms are the melancholias. As well as the typical melancholia, there are two other forms to which we would draw attention, namely, the inhibited and the anxious. Besides vital sadness, I feel that inhibition and anxiety should be included in the clinical picture as primary elements. This would explain why there are typically inhibited depressions without sadness.

The depressions with anxiety bridge the transition between the typical depressions and the clinical picture of vital anxiety. Leonhard has attempted to isolate an "idiopathic anxious melancholia" which is clinically distinguished from the classical melancholia by the presence of anxiety and which furthermore presents certain hereditary characteristics and constitutional peculiarities. Yet there are patients in whom the phase at times appears as one of anxiety and at other times as pure depression or even an inhibited depression. This suggests that in enumerating these clinical pictures we should not proceed to set up independent disease types but to establish clinical types distinguished by the accentuation of one symptom or another within the same syndrome.

2nd. *Anxiety timopathy.*—The term timopathy suggests the affective nature of the illness, and the epithet "anxiety" serves to point out that vital anxiety is its fundamental or primary symptom. Anxiety timopathy does not totally undermine or destroy the concept of anxiety neurosis. Just as there are reactive melancholias characterized by a sadness due to some external event and hence psychogenically produced, there are also reactive anxiety states. The causal conflict may be conscious or unconscious, external or internal, but the presence of a causal situation or conflict is required in order to establish this diagnosis. With the separation of anxiety timopathy based on a nucleus of vital anxiety, the scope of anxiety neurosis is markedly reduced. Evidently there are transitional forms, and a very important problem in the individual analysis of the clinical cases consists in discerning up to what point the anxiety is vital, and to what extent personal or reactive factors are involved.

3rd. I propose to consider in this group those cases called *chronic fatigue*. The patients feel completely overcome by a vague sensation of fatigue not caused by any external agent. This problem has been studied from many points of view and each study has arrived at some truth. Undoubtedly, there are cases of fatigue due to exhaustion especially when the organism functions under extraordinary circumstances like war. There is also such a concept as constitutional fatigue, a manifestation of the constitutional variant of the personality known as the asthenic personality. This variant should not be conceived as projected solely on the somatic plane as in the old constitutional asthenia of Stiller, but also on the psychic plane. But aside from these two groups there exist others probably forming the majority, characterized by fatigue that appears periodically. They last during certain periods of life, disappear and perhaps reappear after several years. Apart from the symptomatic characteristic, this type of fatigue shows other features in its course which brings it closer to the phasic diseases. It is a fatigue that presents itself in the morning following a night's repose and diminishes in the course of the day even though the patient may have been active. The early evening hours are the best for the patient. Now if this were an exogenous fatigue, his feeling would run a contrary course. If it were subjected to a more detailed analysis, its vital character would be more easily recognized.

4th. I propose to include in the timopathic group those typical cases of *organ* or *visceral neurosis*. In the same manner that the depressions with anxiety form the transitional span between the typical depressions and the anxiety timopathy, the organ neuroses form the transitional bridge between anxiety timopathy or typical vital anxiety and the true neurosis. There are two primary reasons why the visceral neuroses come closer to anxiety timopathy, namely: (1) their phasic course in many cases; (2) the prevalence of anxiety as the primary symptom in the clinical picture. As a general rule, anxiety is the dominant symptom in cases of respiratory, gastric, cardiac neuroses, &c. In neurocirculatory asthenia one of the basic symptoms is anxiety. A detailed analysis of the genesis of this anxiety shows that, in general, anxiety is primary, unmotivated and not attributable to an outside situation or an internal conflict. Of course, one commonly meets cases in which the patient has subsequently developed a *secondary anxiety* produced by the disease itself or rather by a reaction of the Ego or the personality. Between the two (primary and secondary anxieties), a true vicious circle may well be set up. (3) The primary character is the oft-repeated fact that visceral symptomatology imputed to a certain organ may in another phase be manifested in another organ or on a different somatic plane.

There is a strong suggestion from the results of clinical observations and therapeutic results that the vital alterations may be linked one way or another with diencephalic regulation. Often, alterations of instinctive life whose diencephalic regulation is evident appear in all these groups. The melancholic's sensation of dryness of the mouth is a mild expression of the disturbance in the subjective plane of the instinct of thirst. The sexual alterations are obvious. In many cases I have observed instability of weight of a cyclic character the diencephalic origin of which appears to be clear.

Treatment.—If the majority of the cases of anxiety were grouped under anxiety neurosis or anxiety reaction, it would be logical to think that the most adequate treatment is psychotherapy. Among the various forms of psychotherapy, psychoanalysis has undoubtedly been the most successful. Apart from these cases, however, there are other forms of anxiety which are no more than mere symptoms of a psychotic crisis, for example, the anxiety accompanying some initial clinical pictures of schizophrenia, the above-mentioned forms of anxiety-melancholia and the anxiety crisis of cerebral traumata under certain situations of conflict. In each of these cases the most adequate and rational therapy would depend upon the fundamental disease, one of whose manifestations is anxiety.

I would suggest that the majority of cases ordinarily diagnosed as anxiety neurosis are of somatic origin. In our experience a somatic treatment rather than psychotherapy will give much better results.

In evaluating the therapeutic results of all anxiety cases, one should bear in mind the phasic course of the majority, and their tendency to a spontaneous remission. This explains many successes credited to psychotherapy. The disease continues to evolve during the period of psychotherapy or psychoanalysis so that the active phase may remit spontaneously at any moment. Naturally the psychotherapist supported by his convictions of the efficacy of psychotherapy concludes that the successful outcome is due to his efforts. I personally have had this experience.

Nevertheless, psychotherapy has its own particular merits. It prevents the crystallization of anxiety into symptoms like phobia and the setting up of vicious circles. Furthermore, in every case it lends support during the whole critical course to the patient, who oftentimes cannot be restored. Psychotherapy should always be maintained even after a somatic treatment has been decided upon.

I have attempted elsewhere to demonstrate how the phasic course appeared in the first clinical histories published by Freud with the diagnosis of Anxiety Hysteria. I refer particularly to the case of Emy found in the Complete Works of Freud. After Freud's first treatment the disease recurs twice and in each recurrence the phobic content changes. Freud himself implicitly admits the presence of an extra-psychological factor which he calls neurotic, in the consideration of this disease.

Starting out then from the vital character or the basic biologic structure of this disease and the established analogy between anxiety timopathy and endogenous melancholia, it was logical to think that in both, identical treatment could be utilized. The excellent results obtained by treating the majority of melancholics with convulsive therapy are well known. Yet, there were two main reasons against the use of electro-convulsive therapy in many cases of anxiety timopathy, namely: (1) there is a disproportion between the intensity of the treatment and the disease to be treated; (2) the very presence of anxiety in the patient caused a repugnance to this type of treatment. Certainly that which subjectively provokes greater suffering to the patient, who is a victim of anxiety, is precisely the fact that electro-convulsive therapy produces a dissolution of the continuity of consciousness. The physical discomforts, including amnesia, following convulsions are of minor importance to him. However, in cases of deeply-rooted anxiety, pertaining more to the group of anxiety-melancholy than to anxiety timopathy, electroconvulsive therapy is indispensable. In such cases we prefer to administer the treatment at bedtime. Pentothal favours the onset of sleep and, after electroshock, we use a strong dose of escofedal (morphine-scopolamine) or a barbiturate causing a deep slumber lasting till the next morning. Therapy is thus made more bearable.

The Use of Acetylcholine

After a search for a more useful and expedient treatment of anxiety timopathy, I decided on acetylcholine. Fiamberti had proposed the treatment of schizophrenia with convulsive doses of acetylcholine. He utilizes the bromide (pragmoline) in the dose of 0·6 gramme. Injected intravenously

and with adequate rapidity of injection he obtained a convulsion with loss of consciousness, preceded by vegetative phenomena corresponding to intense parasympathetic stimulation. The patient develops sudden violent cough, precordial oppression, lacrimation, facial rubor, &c.

In anxiety timopathy, the treatment should start with small intravenous doses which are progressively increased depending on the tolerance of the patient, until a convenient vegetative shock including a transient loss of consciousness is produced. If this progressive dose is given at proper intervals, the patient soon loses his apprehension and tolerates it perfectly. He should be properly informed of the discomforts he may experience. The sensitivity of each patient must be determined with the first injection. The two main points to bear in mind are: (1) amount of acetylcholine injected; (2) rate of injection. By varying these two factors we can modify the observable reaction.

The first injection must thus be tentative, the doses being one-quarter of the maintenance dose (0·2 gramme) over a period of time of a minute. The reactions consist in: difficulty with breathing felt by the patient as a feeling of oppression in the chest, cough, flushing sensation in face and limbs mainly. The pulse slows down and, at the peak of the reaction, may stop for a few seconds, and then rapidly recover the normal rate. The effects of the injection vanish quickly and the patient can get up five minutes after the pulse-rate becomes normal. The dose is increased by a quarter in each following injection until the full maintenance dose is reached (0·2 gramme). The rate of injection is made more rapid up to 20–30 seconds varying according to the sensitivity of the patient.

The injections are given twice or three times a week; in severe cases they can be given daily. We ordinarily do not use the doses indicated by Fiamberti for schizophrenia. We prefer acetylcholine chloride (Roche). It is convenient not to fix the total number of injections. Many cases, however, recover with a treatment consisting of three injections a week for twelve weeks.

Acetylcholine may act mainly as a central diencephalic stimulant. It has been several years since acetylcholine was administered intramuscularly for the symptomatic treatment of anxiety on the theory that the anxiety crisis is of sympathicotonic nature and that acetylcholine has a vagotonic action. However, the effect if given intramuscularly is nil. On the other hand I have tried a combination of acetylcholine and prostigmin intramuscularly with the aim of avoiding its rapid decomposition, thus giving time for its parasympathetic stimulating action to be effective. The results have not been convincing. We have also tried the intravenous use of acetylcholine following an intramuscular injection of Prostigmin. In these cases the vegetative shock is so intense as to produce a true acetylcholine-induced collapse and yet the results are no better. For this reason I have refrained from continuing this technique and I have returned to the standard regimen I mentioned. This standard technique is simple and involves no risks. There is no need for hospitalization and the treatment can be carried out in the patient's home. The injection is given before breakfast or dinner, that is, when the stomach is empty.

In an estimated number of about 12,000 injections, we have never had a single accident. Some physicians believe that acetylcholine, having been used as a hypotensive drug, would be contraindicated in patients with hypotension. As I see it, hypotension is not an independent syndrome but in many cases one of the manifestations of a clinical picture of vital asthenia. As the whole syndrome improves with this treatment, the hypotension is relieved. In some cases, however, the syndrome of vital asthenia improves but the hypotension persists though the patient is unaware of it.

In evaluating the results obtained with the above therapy and in making a catamnesis of cases treated several years back, we should bear in mind certain aspects. If the tendency to a phasic course is very evident in the cases, the phases may lessen the good percentage of recoveries. Dedichen, after a study of the results obtained with electro-convulsive therapy on melancholia, has concluded that the result is nil. This conclusion would be true if the therapy is evaluated from the viewpoint of avoiding fresh attacks. It is false, however, if we consider the therapeutic efficacy on each phase. Is it not worth while to shorten to two or three weeks a melancholia destined to last six, eight or ten months? Is it nothing to give relief on the third or fourth therapeutic application to a despondent patient who is furthermore exposed to the danger of suicide?

In timopathy a similar improvement occurs, but in many cases the phasic character is not as deeply rooted as that of melancholia. After several years the difference between the treated and the untreated is very evident. As already mentioned, the rapid curtailment of anxiety impedes the formation of symptomatic patterns and the setting up of a vicious circle which makes later therapy difficult. There is a great deal of difference between treating an agoraphobia in its first few weeks of development and initiating the treatment several years later after the pattern of life has been built upon the agoraphobia.

Recently I have made a study on patients treated five years ago. I took the first 100 cases diagnosed as anxiety timopathy in 1945. Of these 100 patients, 19 refused treatment. Of these 19 cases 3 recovered spontaneously; 3 improved sufficiently to carry on a normal life; the remaining 13 are still in the same condition as when first seen.

Of the 81 cases treated, 31 recovered completely with no recurrence; 13 had recurrence within a period of one to three years subsequently followed by recovery; 18 improved sufficiently to carry on a normal life and resume their jobs; 19 had no improvement. Of the 13 cases with recurrence, 5 were treated again and they recovered; in 6 others the symptoms were so mild that the patients were not hindered from carrying out a normal life; the 2 remaining who did not come back for

treatment continue to be in the same state. We can count on a 60% recovery. Of the 40 remaining patients, 19 refused treatment, 19 did not respond and 2 had a relapse after the first treatment.

If the groups of cured and unimproved cases are compared, we find that—

In the recovered cases: (1) There are more women than men (22 to 19). (2) There are about the same number of asthenic as pyknic types (10 and 11). (3) There are 11 cases whose course is less than a year, and 3 cases that do not exceed three years. (4) The symptomatology is acute (anxiety crises and phobias).

In the unimproved cases: (1) 12 are males and only 7 females. (2) The pyknic body build is predominant. (3) There are only 3 cases whose course is less than a year while there are 13 lasting more than three years, 10 more than four years. (4) The symptomatology is more somatic and less acute.

The symptoms that recur more frequently and are more resistant to treatment are headache, nuchal paresthesias and obsessions.

TABLE I.—SUMMARY OF RESULTS

		Recovered	Improved	Unimproved	Total
Not treated ..	..	3	3	13	19
Treated	..	44	18	19	81
	Total	47	21	32	100

$$\chi^2 = 14 \cdot 888 \qquad P = 0 \cdot 01$$

The difference between both groups is real and attributable to treatment.

TABLE II.—CONSTITUTIONAL TYPES AND TREATMENT

		Asthenic	Pyknic	Mixed	Athletic	Dysplastic	Total
Recovered..	..	10	11	8	1	1	31
Not improved	..	3	11	3	1	1	19
	Total	13	22	11	2	2	50

$$\chi^2 = 3 \cdot 359 \qquad P = 0 \cdot 65$$

Conclusion.—The results of therapy do not depend upon the constitutional type of body build. However, from my experience over a wide range of patients I feel that pyknics respond better to treatment than asthenics.

In all these cases we did not use any special form of psychotherapy. Undoubtedly in some cases with persistent symptoms, we would bring about a better adjustment of the patient to life by completing the biologic treatment in conjunction with psychotherapy.

Acetylcholine is not the only drug we have used. In my clinic we have tried other preparations always bearing in mind the diencephalic genesis of vital anxiety and the possibilities of treatment by means of vegetative stimulants on the diencephalon. In the last two years we have tried the intravenous use of ammonium chloride and picrotoxin, nicotinic acid and other drugs in an attempt to establish different indications according to the symptomatology. We have not, however, yet obtained precise therapeutic criteria. Ammonium chloride has given encouraging results but we have discarded picrotoxin.

In conclusion, we believe that there are many possible avenues of treatment if we consider that numerous cases of the so-called anxiety neurosis spring from an endogenous and vital basis even when they are accompanied by superimposed psychogenic symptoms.

Dr. William Sargant: Those of us who heard Professor Ibor talk on "The Selective Indications for Shock Treatment" at the last International Congress of Psychiatry in Paris, in 1950, are already aware of the detailed study he has made of many aspects of biological treatment in psychiatry.

I was interested in his observations at that meeting that some patients with anxiety states could be helped by a modified form of Fiamberti's acetylcholine shock treatment for schizophrenia, a technique hitherto neglected in this country. When invited to visit Spain and Portugal last spring, I was able to see this treatment at Professor Ibor's clinic in Madrid, and on my return we decided to experiment with its use at St. Thomas's Hospital. Since July 1951 five out-patient treatment clinics a week have been organized for this treatment. These have been mostly carried out by Dr. R. M. Phillips, working with a whole-time clinical research grant for the investigation of such problems. He has provided the statistics I shall quote and has shouldered the main burden of starting this work. Dr. E. D. Barlow, on another whole-time research grant, is working in conjunction with the Medical Professorial Unit at St. Thomas's on some of the physiological problems involved in this and other shock therapies. Dr. J. T. Hutchinson has also been helping with some EEG and ECG studies on patients undergoing treatment.

The details of the technique have been described by Professor Ibor and those who wish to can see it at St. Thomas's Hospital. I should mention, however, that we wasted quite a lot of time till we confirmed Professor Ibor's observation that the Roche preparation of acetylcholine chloride, which has to be mixed just before injection, seems the most satisfactory one for treatment purposes, as opposed to other preparations available already made up in solution; these seem to produce much more variable reactions and, possibly due to a preservative in the solvent, may thrombose the veins with repeated intravenous injection.

We have now given over 1,400 injections to more than 70 patients. We have had to stop treatment on only 2 patients because of apparent growing hypersensitivity to the drug. Bad reactions can generally be controlled

by lowering the dose, or slowing the speed of injection; and, as reported by Fiamberti, Ibor and others, it has not as yet appeared to be unduly dangerous in trained hands. For instance, 12 patients aged 45 or over have been treated without apparent ill-effects except for hypersensitivity in one, the oldest being a male patient of 61 years of age who has had 19 injections, while a female patient of 59 has had 63 acetylcholine shock treatments. At another hospital to which I am attached, however, a mild coronary thrombosis has recently occurred in one patient four hours after injection though the relationship to the treatment is uncertain. One should obviously be cautious in the presence of heart disease, and a preliminary ECG seems advisable in the older age group when in doubt.

Physiological effects can certainly be marked. (Slides were projected showing temporary stoppage of heart and alterations of brain waves.) Fortunately these effects are generally over very rapidly, as the acetylcholine injected is quickly destroyed by cholinesterase in the blood-stream.

At first we tried the method on a random sample of anxious and hysterical patients, to try to find if any particular group of them might be helped by it, and to get as early an idea as possible of those unlikely to be helped. We were frankly disappointed with the initial results obtained. It has only been with a better selection of patients, based on our early findings, that results have become more interesting and, research funds permitting, we shall try to continue this work till we are quite certain whether or not it really fills a treatment-need not provided by other physical or psychological treatments already available. At present we are inclined to think that it might prove to do so in, at any rate, a small group of patients.

I shall summarize the early impressions of those of us working on this problem: Crude statistics so far suggest that 25% of the first 64 patients have been greatly helped and another 20% improved. Of the remainder, 20% are, to all intents and purposes, unchanged, another 10% showed temporary improvement followed by relapse, despite continuation of treatment, and 22% refused to continue, generally after one or two treatments. The remaining 3% consist of a patient admitted to hospital after 4 treatments and one who stopped attending after 5 treatments.

Studying the results clinically, we have not so far found that it helps the hysterical patients more than any other new treatment with a highly suggestive value. It has not yet been of very much help to those complaining of phobic and anxiety symptoms and also having an immature personality and an asthenic habitus—a syndrome we so often label as anxiety hysteria. Many of these patients have become more frightened by the treatment reactions than by their normal attacks of panic.

Where we have become interested in its results at the present time is in a small number of what can best be described as severe tension or phobic states in mildly obsessive personalities. These patients do not resort to hysterical dissociation as a way out of their problems, and if depressed are anxious rather than retarded, and so rarely do well with E.C.T. They may have driving personalities, and generally a pyknic or athletic build. They put up a good fight against their symptoms, and try to stay at work in spite of them. One particular phobia or rumination may be creating severe anxiety, or their anxiety symptoms tend to become centred mainly on one topic. They may, for instance, have irrational fears of fainting in the street or at work, or be worried about an idea which they know is absurd in their more normal moments. One patient, for instance, could not get the word "syphilis" out of his mind, another was continually obsessed by thoughts of his wife's possible infidelity some years ago although he knew it was not really so; a third had phobias of harming his child, a fourth attacks of unexplained tension as he went about his daily work. Symptoms tend to vary in intensity, and it is interesting to find more patients of pyknic constitution in the groups showing improvement than in the groups not helped. As I have stressed, these are not patients usually helped by E.C.T., but because of their constitution may be liable to mood swings of tension and reactive depression.

It is broadly this particular group of patients that seems so far to have been helped sometimes by acetylcholine even after long periods of illness, and the failure of other methods, including psychotherapy, sedation and E.C.T. to help them. Such patients must, however, be distinguished from classical and full-blown obsessional neuroses with mental rigidity and presenting a host of varied obsessional symptoms and compulsive motor patterns of behaviour.

Another unexpected clinical finding has been at least a temporary relief of head pains associated with states of tension in some patients. After injections, these may ease dramatically, though they generally return again after a short time. Later in the course of treatment, they may become less severe and better tolerated. Two patients with distressing sub-mammary pain have also been helped.

Fiamberti has suggested that acetylcholine shock treatment helps thought disturbance in schizophrenia as opposed, for instance, to associated depressive trends, better treated by E.C.T. We have not been treating schizophrenics, but some of our patients have spontaneously commented on their increased clarity of thought, when helped by treatment, and a diminution of muddled thinking which accompanied their tension symptoms. One patient, unable to concentrate on reading for a year and a half, was able to read two volumes of Churchill's Memoirs in a fortnight.

While some relief may be experienced after a small number of injections, it has proved well worth while to continue in many cases for up to 40 or more injections if improvement is being obtained. Fiamberti recommends over 100 treatments in patients with schizophrenia. But we have not reached this number in any of our neurotic patients so far. Generally, we try to give the treatment three or four times a week to start with and reduce the frequency later. Some patients seem to be helped by occasional maintenance treatments. There seems to be no need to provide too severe an acetylcholine shock reaction to get results. But those benefiting do not appear to be unduly alarmed if this occurs and still continue to attend—in marked contrast to the behaviour of hysterically anxious patients, who become so easily upset with their first severe acetylcholine reaction.

St. Thomas's is centrally placed in London and, now having a specially equipped and staffed out-patient treatment unit with its own waiting-room and rest cubicles, &c., this allows a whole variety of such treatments to be given on an out-patient basis instead of needing hospital admission. Patients therefore get off from work for an hour, have this treatment, rest if necessary, and then return to work again. We hope in this way to be able eventually to assess its value with greater certainty than we can do at present and report our final results after a longer follow-up period.

Section of Odontology

President—R. A. Broderick, D.S.O., M.D.S.Birm., F.D.S., M.B., Ch.B.Birm.

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Laboratory Investigations on the Relation of Fluorine to Dental Caries on Tyneside and District

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Two main theories have been suggested to explain the anti-caries action of water-borne fluorine, but the evidence for neither is conclusive. Both these theories assume that acid is the attacking force in caries, and it cannot be said that any plausible suggestions supported by experiment have yet been made that would explain the action of fluorine should the proteolytic theory be correct.

The first theory is that fluorine enters the mineral matter of the enamel and dentine and thereby reduces the acid-solubility of the tooth.

Several workers have published data which show that the concentration of fluorine in enamel and dentine is higher in areas with a high level of fluorine in the drinking water (for review and references *see* McClure, 1948). McClure and Likins (1951) have presented detailed figures showing this relationship over a wide range of fluorine concentrations, including the levels 1–2 ppm. (parts per million) at which the fluorine markedly reduces caries but causes little mottling.

Volker (1939) has shown that when powdered enamel or dentine from human or rat teeth is treated *in vitro* with high concentrations of fluoride, its solubility in acetic buffer at pH 4·0 is reduced. Similar tests on powdered enamel from *naturally* fluorosed human teeth and from teeth of rats receiving 20 ppm. of fluorine in their drinking water (this concentration in water is required for an anti-caries effect in rats) failed to reveal any difference in solubility. No information was given of the level of fluorine ingested by the human subjects or of its concentration in the teeth, except that they came from an area showing a mild degree of mottling.

We are aware of only one other investigation on the acid solubility of naturally fluorosed human enamel, that of McClure and Likins (1949). These workers measured the solubility in several acids of powdered enamel pooled from small numbers of teeth, and found that the fluorosed samples (fluorine content between 0·0618 and 0·0909% suggesting a level in the water of at least 4 ppm.) were less soluble in 0·01N solutions of organic acids, but no difference was found with hydrochloric, nitric and phosphoric acids.

We have found no record of tests on the solubility of intact human teeth from populations whose fluorine intake from water and average caries incidence are known, and it is this information which would seem to be desirable to test the theory.

The second theory, that fluorine exerts its protective action against caries by antibacterial mechanisms, is based on two types of observations. Firstly, it has been stated that *Lactobacillus acidophilus* counts of several thousand saliva samples were lower in "high fluorine" than in "low fluorine" areas (Dean *et al.*, 1939, 1941; Jay, 1942; Jay and Arnold, 1946). Secondly, Bibby and his colleagues have shown that fluorine influences the metabolism of salivary bacteria (Bibby and Van Kesteren, 1940; Bibby, Volker and Van Kesteren, 1942). In their first paper these workers investigated the influence of different concentrations of sodium fluoride on the growth and acid production of many strains of salivary organisms, and found that with streptococci the level of sodium fluoride required to produce the smallest perceptible decrease in acid production was about 2 ppm. and that much larger amounts (between 250 and 1,000 ppm.) were required to affect growth.

The figures of Bibby and Van Kesteren on lactobacilli are in one respect somewhat puzzling. In tests on 10 strains of lactobacilli in a medium to which 2 ppm. of NaF had been added, they found a *larger* reduction in the acid production (45%) in 24 hours than they observed at any of the other levels tested (10, 20, 50, 100 and 250 ppm.) except 1,000 ppm. No comment is made in the text on the anomalous behaviour of the lowest level of fluorine tested.

Cox and Levin (1942) also state that, "in preliminary experiments" with a medium containing about 0·1 ppm. they have found that "the addition of very small amounts of fluorine at that level does have remarkable inhibitive effects on acid production", but no confirmatory data appear to have been published.

Since the question of the level of fluorine needed to exert significant antibacterial effect is unsettled it obviously requires reinvestigation. Estimates of fluorine concentration of saliva range between 0·35 and 0·08 ppm., and have not been shown to vary with the level in the drinking water (Cox, 1940; McClure, 1941; Martin and Hill, 1950).

There is clinical evidence that the anti-caries effect of fluorine in tap-water depends upon its

administration while the teeth are being calcified (Dean *et al.*, 1941) and the effect remains even if the water supply or place of residence is changed (Dean *et al.*, 1941; Deatherage, 1943). These observations imply that the effect of fluorine is "built in", presumably to the teeth, and does not require continuous exposure to water containing fluorine as would be expected if salivary fluorine or the fluorine of the water were itself directly concerned in the inhibition.

Bibby *et al.* also tested the effects of fluorosed dental tissues on the growth and metabolism of certain salivary bacteria. Their technique consisted of shaking 200 mg. samples of ground enamel or dentine with pure cultures of the organisms at 37° C. in suspensions of 10 ml. of broth for periods up to 96 hours, and measuring the acid produced, by colorimetric estimation of the pH and by titration against 0·1N NaOH. They found that the presence of the enamel and dentine altered the acid production; with some organisms more and with others less acid was produced than in the absence of dental tissues. When fluorosed enamel and dentine were present in cultures of organisms whose acid production was increased by dental tissues, the increase was smaller than with normal unfluorosed tissue and if the calcified tissue reduced acid production, then fluorosed material caused a larger reduction. The fluorosed tissues used were obtained either from rats which had been fed a high fluorine diet or by treatment of rat and human tissues with sodium fluoride solution *in vitro*, and they state: "Unfortunately the fluorine content of the teeth tested was much greater than has been encountered in human teeth and so the reasoning does not necessarily apply to the human mouth."

The exact nature of the increase and reduction of titratable acidity by dental tissues observed by Bibby *et al.* was not decided by these workers. The fact that heavily fluorosed tissues caused a lower titratable acidity was not fully discussed, but in their first paper they refer to it as an inhibitory action and it has been assumed by other writers that fluorosed enamel and dentine can exert an antibacterial action. In their second paper, they avoid any clear statement about the different behaviour of fluorosed and non-fluorosed tissue, but suggest that the general effect of calcified tissues on titratable acidity is caused by buffering power. They imply that as the tooth substance dissolves the phosphate concentration and therefore the buffering power of the medium rises, and, depending upon whether the pH at which it is buffered is favourable or otherwise for the organism studied, more or less acid would be produced. It is important to emphasize the fact, not mentioned by Bibby *et al.*, that increase in buffering power would in itself increase titratable acidity even if it had no influence on the rate of metabolism of the organisms.

It was shown by Bibby and his colleagues that the fluorosed tissues which they used were less soluble than the normals and this means that they would, in the experiments with organisms, contribute less buffering power and therefore exert a smaller influence on titratable acidity. In other words, the apparent effect of the fluorosed tissue on acid production might be attributed to a direct result of their reduced solubility and they might be exerting no true anti-bacterial action whatever. In addition to this possibility, the buffering action might favour acid production by either intra- or extra-bacterial enzymes, or might affect the survival rate of the organisms. They did show that with one strain of streptococcus the death-rate of the organism was slowed in the presence of the tooth substance and suggest that this partly accounted for the increased acid production.

In view of these uncertainties, it seemed desirable to reinvestigate these experiments from two points of view. First, to find out whether the effects of dental tissues on bacterial acid production are entirely due to changes in buffering action altering titratable acidity, or whether any influence is exerted on their metabolism and, if so, to ascertain whether fluorosed tissues obtained from human teeth of residents in a "high fluorine" area are sufficiently different from normal tissues to show any of the effects observed by Bibby *et al.*

Another possible explanation of the action of fluorine was raised by Clapper (1947) who observed that when a strain of oral lactobacillus was grown in a medium containing 100 ppm. of NaF for six months, and then transferred to a fluoride-free medium, the power of acid production was greatly reduced and remained so for up to six months on the fluoride-free medium. Growth, as judged by visual inspection of opacities, was unaffected. This effect was not observed with a medium containing 1 ppm. NaF, and no experiments were carried out in the presence of fluorosed tissues. It would seem unlikely that these observations are relevant to the caries-fluorine problem because higher concentrations of fluorine are apparently necessary than are likely to be present in the mouth, nor would it explain the low lactobacillus counts which have been reported in high fluorine areas.

When the present work was begun in 1948 it was based on the figures of Weaver's 1943 survey showing that both 5-year-old and 12-year-old children in S. Shields had a DMF[1] figure about 40% less than had N. Shields' children (Weaver, 1944*a*). The survey of 1949 showed that the "wartime diet" had altered the picture; among the 5-year-old children the DMF figure had fallen by 33% in N. Shields but only by 10% in S. Shields and the DMF figures for this age-group were no longer strikingly different (Weaver, 1950). Among the 12-year-old children the DMF figures had fallen about equally, however. Weaver also published figures for the DMF among W. Hartlepool children (in this town the water contains 2 ppm. of fluorine), and found a large difference when compared with the N. Shields' figures. These results are summarized in Table I.

[1]DMF—Decayed Missing Filled Teeth.

TABLE I.—DMF OF SCHOOLCHILDREN IN RELATION TO FLUORINE OF WATER
(Data from Weaver, 1950)

	F. of water (ppm.)	DMF			
		5-year-old		12-year-old	
		1943	1949	1943	1949
N. Shields ..	<0·25	6·6	4·4	4·3	2·4
S. Shields ..	1·4*	3·9	3·5	2·4	1·3
W. Hartlepool	2·0	—	1·76	—	0·96

*Forrest, Parfitt and Bransby (1951) reported a lower figure, viz. 0·82.

In view of these clinical findings it was decided to abandon the comparison of N. and S. Shields and to repeat the investigation on N. Shields and W. Hartlepool, and it is these results which are mainly given in this paper.

EXPERIMENTAL
Solubility of Teeth

Methods

(a) *Intact teeth*.—Teeth were obtained from the School Dental Clinics. After extraction, the teeth were washed free of blood and saliva, wrapped in moist cotton-wool or gauze and placed in a corked specimen tube. The interval between extracting and testing was rarely more than a few days. A form was filled in at the time of extracting giving date of extraction, age of child and stating whether the child had been born and had always lived in that particular town. The specimens were not used if the child had been born or had lived elsewhere.

On arrival at the laboratory the teeth were treated as described by Brudevold (1948), that is, after scrubbing with a toothbrush the teeth were mounted in a cuboid of wax, except for a window 4 mm. in diameter. It was found to be most important for the overall sizes of the waxed teeth to be identical as any variation altered the efficiency of shaking and the amount of tooth substance dissolving. Each waxed tooth was then placed in a tube containing 5 ml. of 0·2 M acetate buffer at pH 4·0 and mechanically shaken for 10 minutes at 37° C., after which the tooth was removed, blotted dry with filter paper, and placed in another tube with fresh acid and shaken for a second period of 10 minutes. The amount of P dissolved by the acid buffer was estimated on 4 ml. samples by Briggs' method. In some of the later experiments, 0·5 ml. or 2 ml. samples were taken in duplicate and estimated by the much more sensitive method in which stannous chloride is used as a reducing agent. The depth of colour was measured in the majority of experiments by means of the Spekker photo-electric colorimeter but a few determinations were made with the E.E.L. colorimeter.

Ten teeth were studied at a time, five from N. Shields and five from either S. Shields or W. Hartlepool. In each experiment, the two sets of teeth were matched as nearly as possible from the point of view of morpho-logical type and the age of the subject. The teeth were studied in several series, the first consisting of 157 deciduous teeth from N. and S. Shields, the second of 105 deciduous teeth from N. Shields and W. Hartlepool, and the last of 76 teeth from adults in N. Shields and W. Hartlepool. The results from each series are not necessarily comparable, but great care was taken to ensure that the teeth were as comparable as possible within each series.

(b) *Ground enamel*.—Batches of 10 mg. of the ground material prepared by the method of Manly and Hodge (1937) from the same teeth used previously, sieved to the same range of particle size (100 to 150 mesh) were shaken with 3 ml. of acetic buffer (pH 4·0) at 37° C. for 10 to 20 minutes and, after allowing the solid to settle, the fluid was decanted and the dissolved P was estimated as in the experiments on whole teeth.

Results

The results of the solubility tests on the deciduous teeth, by Brudevold's method, are set out in Tables II and III. They show that the average amount of P dissolving from the N. and S. Shields

TABLE II.—SOLUBILITY OF DECIDUOUS TEETH

	North Shields	South Shields	% Diff.
No. of teeth ..	77	80	
Av. P dissolved (μg.)			
1st period ..	10·4	9·2	12
2nd period ..	16·3	13·3	18

TABLE III.—SOLUBILITY OF DECIDUOUS TEETH

	North Shields	West Hartlepool	% Diff.
No. of teeth ..	52	53	
Av. P dissolved (μg.)			
1st period ..	11·6	6·8	42
2nd period ..	16·4	8·1	50

teeth during the first 10 minutes' exposure to acid differ by only 12%, a difference which is not statistically significant (t = 1·48, for significance (p = 0·05) t = 2·0). When the analogous figures for the N. Shields and W. Hartlepool teeth are compared, however, they are seen to differ by 42%, this difference being highly significant, (t = 3·24, for significance (p = 0·01) t = 2·6). The figures obtained confirmed Brudevold's finding of the great variability in the solubility of teeth and also that less P dissolves during the first 10-minute period of exposure to acid (i.e. from the outermost layer of the teeth).

It is realized that the number of teeth in the N. Shields–W. Hartlepool series is small, but the difference was so highly significant that it seemed adequate at the time it was carried out. Two recent developments make it desirable that this work be extended, however. Firstly, prior to repeating the experiments of Bibby *et al.* (1942) on the effect of ground enamel on acid-producing organisms, it became necessary to test the solubility of ground enamel in acids.

It was not surprising to find that no difference could be detected in the solubility of ground tissues from N. and S. Shields teeth, since this agreed with the results on whole teeth, but it was unexpected to find that there was no significant difference when ground enamel from N. Shields and W. Hartlepool were compared (Table IV).

TABLE IV.—SOLUBILITY OF POWDERED DECIDUOUS ENAMEL

	No. of expts.	No. of readings	Av. mg. P dissolved
N. Shields	4	17	1·86
W. Hartlepool.. ..	4	18	1·83

Secondly, at one stage of this work, the supply of extracted teeth from school children in W. Hartlepool proved inadequate to meet the needs of our experiments and it was decided to collect teeth extracted from adults who were born and had lived all their life in the two towns. When teeth from adults of similar age were available simultaneously from the two towns, their solubilities were tested by Brudevold's method before grinding and separation of enamel and dentine. It has not yet been possible to test large numbers of adult teeth because the majority of the extracted teeth from N. Shields came from younger patients than those from W. Hartlepool, and the results must be regarded as preliminary only. The results have shown, however (Table V), that there is no significant

TABLE V.—SOLUBILITY OF TEETH FROM ADULTS

	North Shields	West Hartlepool	% Diff.
No. of Teeth	39	37	
Av. P dissolved (μg.):			
1st period	13·3	12·0	10
2nd period	19·0	17·1	10

difference between the solubilities of the two groups of teeth and even this small number indicates at least that the large difference observed between deciduous teeth from the two towns does not apply to the teeth of adults.

Discussion on Results of Tooth Solubility Experiments

The first point to be considered is the fundamental difference between the two methods of studying solubility and their possible relation to the carious process. Brudevold's method is a test of the solubility of the outermost surface of the teeth and the results will depend not merely on the chemical composition, but also on the presence of any protective layers or sub-microscopic differences in the texture or roughness of the surface. The solubility of well-mixed ground enamel of standard particle size would seem to depend entirely on differences in chemical composition of the enamel as a whole.

The results obtained from studying the solubility of the intact tooth surface suggest that there is a large difference between the solubilities of deciduous teeth from N. Shields and W. Hartlepool and no significant difference between those from N. and S. Shields. As already mentioned, the numbers of teeth in the groups, although small, seemed adequate to establish these facts. The more recent findings that neither the ground tooth substances nor the adult teeth from N. Shields and W. Hartlepool share this difference clearly makes it desirable to extend all these observations before regarding them as final. If, however, we accept them tentatively at their face value certain conclusions seem to emerge. In the first place, it is clear that the reduced solubility of the intact W. Hartlepool deciduous teeth is confined to the surface. Two possible interpretations suggest themselves: either the solubility is reduced because of a chemical difference between the surface and the interior of the tooth (and a higher fluorine content due to direct acquisition of F from drinking water might account for this), or there is some architectural difference between the surfaces of fluorosed and non-fluorosed teeth.

If the solubility of teeth is related to caries at all, the finding that the solubilities of adult teeth from high and low fluorine towns appear not to differ raises the interesting possibility that the reduction in the anti-caries effect of fluorine with advancing age (Weaver, 1944*b*; Forrest *et al.*, 1951) may be related to the loss of this difference in solubility. The smallness of the difference in caries among adults of high and low fluorine areas has previously been interpreted as a gradual reaching, by middle life, of the maximum possible number of cavities—a state of affairs occurring at a younger age in a low fluorine area. These results raise the alternative possibility that the fundamental action of fluorine on solubility is lost as age increases.

The question of the relation of tooth solubility to caries is even more difficult to assess. Whether the differences in solubility which we have observed on the surface of W. Hartlepool teeth apply also to the contact points and the bottoms of the pits and fissures in which caries normally begins is, of course, not known. If the difference is produced by the post-eruptive acquisition of fluorine the areas protected from drinking water and saliva would probably not be altered.

The solubility of enamel in saliva is, of course, much lower than in acetic buffer, and above pHs of about 5·5 it is virtually insoluble. Suess and Fosdick (1951) have suggested that the critical pH at which enamel dissolves will be lower with fluorosed enamel, a view based on their experiments with enamel heavily fluorosed artificially. Stephan (1940) has shown that acid production in the

plaque is a rapid intermittent process and that when the pH falls below the critical figure it does so for a short time only. If the critical pH is lower, then the plaque will fall below this pH less often and for shorter periods which, on the acid theory of caries, would delay the formation and extension of cavities.

The results on the solubilities of teeth from the three towns suggest that at levels of fluorine intake of 2 ppm. or thereabouts, reduced solubility may play a part in the anti-caries action of fluorine as described by Suess and Fosdick. At levels of 1 ppm., however, our results suggest that the differences in solubility are too small to be significant. It must be emphasized that these results in no way *prove* that reduced solubility is one of the mechanisms by which fluorine reduces caries, but merely show that, at the level of intake of 2 ppm., this theory may be correct.

We have no explanation to offer for our finding that adult teeth from N. Shields and W. Hartlepool do not differ in solubility. If fluorine is constantly entering a tooth in a high fluorine area, as is often supposed, it would have been expected that differences in solubility between teeth from high and low fluorine areas would increase with age. It would be of great interest to compare the solubilities of permanent teeth from young subjects of the two towns, but this has not so far been possible because so few teeth of this type are extracted in W. Hartlepool.

The Antibacterial Action of Fluorine

Methods

Saliva samples from children living in N. Shields and W. Hartlepool were collected by the following procedure. The subjects placed a piece of wax in their mouths, and warmed it for one minute before beginning vigorous chewing for five minutes, during which the saliva was collected in numbered specimen tubes. The tubes were securely corked and returned to the laboratory as quickly as possible. Forty children of the same age-group from each town took part in the test on each occasion. The collections took place simultaneously in the two towns about two hours after lunch.

The acid production of the saliva was then measured by a modification of the test of Wach *et al.* (1943).

In the first series of 229 samples the test was carried out immediately on arrival at the laboratory as follows. Each saliva sample was thoroughly shaken and 2 ml. were pipetted into a test tube containing 0·5 ml. of glucose; the two fluids were not mixed immediately. Samples from each town were pipetted alternately to ensure uniformity of heating and standing. Within a few minutes of the addition of the saliva, a second worker shook the tube to mix the saliva and glucose, and determined the initial pH. Batches of ten tubes were put into the incubator about every 15 minutes at times which were recorded. After incubation for three and a half hours, each batch was withdrawn from the incubator and the final pH determined. By this technique all of the 70–80 samples tested on each occasion were incubated for identical times although the time elapsing between collection and testing of the saliva would become progressively longer. Since samples from each town were set up alternately any changes occurring during the standing (and such changes have been shown to be small) might be expected to affect equally samples from each town.

In the second series, the samples were kept in the refrigerator overnight and were tested the following day. Tests on the samples showed that the relative acid production figures did not change appreciably during 24 hours standing, the only difference being that the initial pH was higher.

The lactobacillus counts were carried out by the technique of Jay and Arnold (1946).

Results

The results of the first series of acid production tests are set out in Table VI and show that there

TABLE VI.—SALIVARY ACID PRODUCTION

		No. of samples	pH		
			0 hr.	3½ hr.	Diff.
First series:					
N. Shields..	..	115	7·49	5·44	2·05
W. Hartlepool	..	114	7·40	5·28	2·12
Second series:					
N. Shields..	..	101	7·80	5·34	2·46
W. Hartlepool	..	100	7·71	5·24	2·47

are no differences either in the initial or final pH's in the groups of saliva from either town. This result was so unexpected, in view of the American work on lactobacillus counts, that it was decided to repeat it on a similar number of salivas and to carry out lactobacillus counts simultaneously. The results on acid production are included in Table VI and confirm the previous finding that there is no difference in acid production or initial pH. The higher average initial pH of the second as compared with the first series is explained by the longer interval which elapsed between collection and estimation.

Although it was realized that the number of subjects was too small to give a decisive answer for so variable a factor as a lactobacillus count it was thought that a difference as large as those reported by the American workers would be observed even on this number.

These limited results, given in Table VII, do indicate that W. Hartlepool saliva samples contain a higher proportion of lower counts and a lower proportion of higher counts than those from N. Shields. These results on acid production and lactobacillus counts suggest that one action of fluorine is to

TABLE VII.—SALIVARY LACTOBACILLUS COUNTS

	No. of samples	Percentages of samples within each range		
		0–100	101–10,000	over 10,000
N. Shields	147	5	27	68
W. Hartlepool ..	141	11	36	53

inhibit the growth of lactobacilli and perhaps other salivary-acid-producing organisms, but the absence of an effect on acid production suggested that there could be no general antibacterial action. As already stated, previous workers have left in a somewhat uncertain state the question of what concentration of fluoride is required to inhibit lactobacilli. It seemed important to reinvestigate the effects of low concentrations of fluoride on acid production by lactobacilli and also by mixed salivary organisms.

Our colleague Dr. D. E. Wright investigated the effect of low concentration of fluoride on the fall in pH of saliva incubated with glucose and found that levels as low as 0·5 ppm. produced a small but statistically significant effect. The average difference in 13 salivas was only 0·03 pH after four hours and 0·05 after twenty-four hours incubation, however, and it seems most unlikely that such a small difference would have an appreciable influence on caries (*see* Table VIII). At a level of 1 ppm. the

TABLE VIII.—EFFECT OF FLUORIDE ON ACID PRODUCTION
(24 HR. INCUBATION)

	No. of tests	0·1	0·5	1·0	10·0
Conc. of F (ppm.)					
Difference in pH:					
(1) Saliva ..	13	0·01	0·05	0·09	0·70
(2) Lactobacilli ..	16	0·02	0·06	0·09	0·13

average difference was 0·09 pH after four hours incubation, again a difference of very doubtful practical value.

There was no evidence of the great inhibition at about 1 ppm. shown by Bibby and Van Kesteren and mentioned by Cox and Levin.

In the experiments on lactobacilli no attempt has been made to study exhaustively the effect of a wide range of fluorine concentration but attention has been focused on the possibility already mentioned that concentrations of 1 ppm. or thereabouts are extremely inhibitory.

Since saliva is a readily obtained medium, which is low in fluorine, it was decided to grow the organism in sterile saliva to which various levels of fluorine had been added. The procedure adopted in a typical experiment was as follows: 250 ml. of saliva from inhabitants of a low fluorine area were divided into five batches to each of which were added 5 ml. of water containing sufficient sodium fluoride to raise the concentration by 0, 0·1, 0·5, 1·0 and 10·0 ppm. of fluoride. The salivas were then sterilized and on cooling 2 ml. of each batch pipetted with sterile precaution into sets of 5 sterile tubes containing 0·5 ml. of glucose solution. Each set of five tubes was then inoculated with 0·1 ml. of a 24-hour culture of a salivary lactobacillus and the pH was determined after 8, 12 or 24 hours incubation.

The average results of 16 tests are given in Table VIII and they provide no evidence whatever that very low levels of fluorine are extremely inhibitory. Concentration of 10 ppm. seems to be necessary before any consistent decrease in acid production occurs and even at this level it is doubtful whether the inhibition would be large enough to have any significant effect on caries.

Scrivener *et al.* (1949) have stated in an abstract that sodium fluoride "in the proper concentration" was found to stimulate the growth of *B. brevis*, the organism which produces the antibiotic tyrothricin. We have found that *B. brevis* is a frequent inhabitor of the human mouth. This suggests that fluorine might produce its antibacterial action, not directly on the caries-producing organism, but by stimulating the growth and presumably tyrothricin activity in saliva of *B. brevis* which would in turn hinder the growth of lactobacilli and other tyrothricin-sensitive organisms.

Tests were made on the influence of low concentrations of fluoride on the growth of *B. brevis* and indications were obtained that at levels of below 1 ppm. growth was stimulated, while higher levels inhibited. Tyrothricin production, however, measured by an unpublished modification of the hæmolytic method of Dimick (1943) was inhibited at all levels. The tyrothricin content of two samples of pooled saliva, one from N. Shields and the other from W. Hartlepool, were also estimated. Both samples contained very low concentrations and there was no significant difference between them. It must be borne in mind that the specificity of the method when applied to human saliva is still uncertain but our figures probably give the upper limit.

These experiments on pure cultures of *B. brevis* and on saliva give no support to the idea that the anti-caries action of fluorine is exerted via tyrothricin.

Discussion of Saliva Results

The observations that the average acid production by salivas from subjects in the high and low fluorine towns did not differ, but that lactobacillus counts did, might be interpreted in several ways.

It is clear that the acid production test is a crude one, affected not only by acid-producing bacteria but also by the buffering power of the saliva, the presence of ammonia-producing as well as acid-removing organisms, and perhaps other factors (Hill and White, 1949). It seems unlikely that in over 200 samples these factors would exactly counterbalance any differences in true acid production and it seems more probable that fluorine exerts its action specifically on only some of the acid producing organisms. Against this possibility we have the evidence in Table VIII that similar concentrations of fluoride are required to bring about a demonstrable reduction in acid production in saliva as in pure cultures of lactobacilli. The difference between the results of the saliva tests and lactobacillus counts is therefore not easily explicable.

In spite of the failure of acid production tests to demonstrate any difference in the salivary activity of samples from N. Shields and W. Hartlepool there can be little doubt from the extensive data of others and our own small survey that the ingestion of fluorine-containing drinking water does lower the acidophilus count, a fact which is likely—though not proved—to indicate that part at least of the anti-caries action of fluorine is exerted through antibacterial mechanisms. Since the concentration of soluble fluoride required to have sufficient antibacterial action to affect caries is high, the question arises: by what mechanism can an effective concentration be built up in the mouth?

It is obvious that the carious attack develops in the dental plaque and it is here also that presumably most of the organisms of saliva originate. The fluoride content of the plaque is unknown and would be extremely difficult to estimate by present methods. It might be speculated that if the aqueous phase of the plaque consists of saliva, then the concentration of fluorine might be expected to be similar to that of saliva, i.e. too low to influence bacterial activity. If, as seems less likely, the aqueous phase of the plaque is drinking water then the fluorine concentration would be greater and perhaps high enough to have some influence on acid production but still not enough to affect the growth of lactobacilli.

The importance of drinking water as a source of fluorine in the plaque could perhaps be studied by experiments with water containing 1 to 2 ppm. radioactive fluorine but the short half-life (112 minutes) has made such a study impossible in Newcastle which is so distant from a supply of radioactive elements.

The Possible Antibacterial Properties of Fluorosed Tissues

If saliva and drinking water seem unlikely sources of antibacterial concentrations of fluorine, there remains the possibility that fluorosed enamel may in some way be toxic to the bacteria of the plaque or saliva, an interpretation which has been placed on the experiments of Bibby *et al.* (1942). As mentioned in the Introduction, this work was capable of a different interpretation and experiments were planned to determine whether all the differences noted by Bibby were explicable as buffering effects or whether there was any evidence of real antibacterial activity.

Experiments based on those of Bibby *et al.* have been carried out but differing in two respects. First, naturally fluorosed dental tissues have been used mostly, and second, the effect has been tested so far only on mixed salivary organisms instead of pure cultures.

Briefly, the results of these experiments showed that no difference was observed when the behaviour of saliva in the presence of ground enamel from N. Shields teeth was compared with that from W. Hartlepool teeth. It is, therefore, irrelevant to discuss whether changes in acid production produced by incubating saliva with enamel are caused by buffering effects.

The fact that no true antibacterial action has been demonstrated by the experiments on ground enamel does not definitely prove that fluorine on a tooth surface has no influence on the organisms of the plaque, because the ratio of tooth to organism in the plaque is clearly quite different from that in the test tube. Also, our finding that the solubility of the outer surface of the enamel differs from that of the enamel as a whole makes it clear that the experiments should be repeated on intact non-carious teeth. Unfortunately, owing to the extreme variability of the solubility and surface areas of different teeth, the results of such experiments are valueless unless large numbers of teeth are used and this has not so far been possible, but is planned for the future.

The clinical results already mentioned do suggest that fluorosed enamel does in some way influence salivary bacterial counts although it is not easy to picture a mechanism by which solid fluorosed material could act antibacterially. There would seem to be only four ways in which this could occur. First, since the concentration of fluorine in the enamel is much higher (perhaps several thousand times higher) than in the saliva, it is possible that fluoride ions may leave the enamel and enter the plaque. McClure and Likins (1949) have stated, in a brief report with no numerical data, that they have demonstrated the removal of fluoride from artificially fluorosed ground enamel by ten washes with distilled water. It must be admitted that it is difficult to see how sufficient concentrations of fluoride could be accumulated in the plaque to affect acid production and bacterial growth significantly, without fairly rapidly depleting the surface of the enamel of fluoride, particularly as on most parts of the tooth the plaque is probably disturbed and re-formed several times a day. It might be argued that only in the sheltered parts of the mouth, where the plaque is undisturbed, i.e. the potential sites of carious cavities, would it be necessary to build up toxic levels of fluoride. Such reasoning does not account for the lowered lactobacilli counts in saliva, however. It must be assumed that a large proportion of the plaque material is affected by fluoride in order to lower the bacterial activity of saliva as a whole.

The work of Volker *et al.* (1940) has shown that radioactive fluoride tends to enter bone, enamel and dentine even from solutions with a concentration as low as 10^{-5} and it has been widely believed that a tooth may increase its fluoride content by contact with fluoride-containing water. The experiments of Volker *et al.*, although proving that radioactive fluoride enters enamel, do not necessarily prove that there is a net gain from solutions of low concentration since no account is taken of the possible simultaneous loss of some of the non-radioactive fluoride already present in the enamel.

The crucial experiment to decide whether fluoride is constantly accumulating at the surface of enamel, or is leaving the enamel and entering the plaque would be to estimate the fluoride at the surface and compare it with the concentration of the mixed enamel from the interior.

We have attempted this experiment by exposing the crowns of 30 W. Hartlepool adult teeth to acetic buffer at pH 4·0 until an average of 1 mg. per tooth of the most superficial enamel had been dissolved away, as shown by P estimations on the buffer. The teeth were then powdered and the enamel separated from the dentine. The fluorine of the surface and interior of the enamel was then estimated separately by the method of McClure (1939) and in the one experiment so far carried out the result showed that the surface contained approximately four times as much as the interior (surface 0·11%, interior 0·023%). Owing to difficulties in the technique and the remote possibility that substances interfering with the fluorine estimation are concentrated on the enamel surface, this finding must be accepted with caution.[1] Nevertheless, it suggests that fluorine enters rather than leaves the enamel.

Secondly, it is obvious that if enamel dissolves in an acid plaque the fluoride would be released with the other constituents of the enamel. For antibacterial concentrations of fluorine to be built up, considerable amounts of enamel would have to dissolve and it seems illogical to postulate an anti-caries action which can only be exerted by dissolving away enamel.

The third possible method by which an insoluble substance could exert an antibacterial action is by absorbing some substance required by the organisms. The known affinity of fluorine for magnesium, which is an important co-enzyme in carbohydrate metabolism, raises the possibility that the fluorosed surface might remove magnesium from the plaque and thus deprive the plaque organisms of this substance. There is no experimental evidence for this view and it is not known whether fluorine bound to the tooth surface (probably as fluoropatite, but this is still uncertain) would have any influence on magnesium ions. In any case it would seem unlikely that the low level of fluorine in the tooth could exert any prolonged action on the plaque. Nevertheless this possibility is one now being investigated.

The fourth possible mechanism is that fluorine alters the micro-architecture or physical properties of the surface of the tooth and makes it in some way less hospitable for the attachment of colonies, but studies with the electron microscope lend no support to this possibility (Gerould, 1945).

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[1]Since this paper was read it has been possible to repeat this experiment on several batches of 25 teeth from adults with similar results.

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Mr. D. J. Anderson: I am very pleased to hear that in his solubility experiments Dr. Jenkins has used intact teeth as well as powdered enamel. Caries starts on the surface of the tooth and therefore a study of the tooth surface seems likely to be a profitable approach. Were the teeth treated to get rid of Nasmyth's membrane, and was the same part of the surface studied in all cases? With regard to the lactobacillus counts, presumably a statistical analysis of the results would be of assistance in deciding whether the difference between the W. Hartlepool and N. Shields saliva samples was significant or not. The contradiction between the results for acid production and lactobacillus counts is not surprising, in view of the variability of lactobacillus counts in the same individual from time to time.

Dr. Jenkins, in reply: The brushing of the teeth would not have removed Nasmyth's membrane: The same surface could not always be used because with carious teeth there is frequently little choice.

Dentine Resorption Repaired with Dentine
By MARTIN A. RUSHTON, M.D., F.D.S.

IT is not very uncommon to find a tooth in which the formation of dentine throughout has been at some time prematurely arrested, where the dentinal tubules cease abruptly along a single incremental line, and where subsequently the formation of dentine has recommenced with the development of new tubules not continuous with the old. These are usually thought to be produced by the original odontoblasts which have recovered, but could also be the result of newly differentiated odontoblasts. In such a case the pre-dentine in existence at the time of the disturbance can often be recognized. It has been covered by a layer of collagenous matrix in which some cells have been included, and on the pulp side of this are found the terminal branches of the new dentinal tubules leading to the pulp. Other teeth are found in which the formation of tubular dentine has never been resumed and a collagenous matrix without dentinal tubules takes its place. This happening can possibly be the result of a general illness of very great severity but is more commonly caused by a local accident, such as partial dislocation of the tooth or thrombosis of its vessels, which temporarily interrupts the nutrition of the pulp. In such a case the pulp cavity may eventually become very small or may be entirely obliterated.

In some of these cases it can be seen that, subsequent to the arrest of dentine formation, there has been resorption of part of the dentine forming the wall of the pulp cavity at that time and that the resorbed area has afterwards been repaired with collagenous matrix having some resemblance to cementum or bone but not to tubular dentine. Similar resorption and replacement are well known to occur in a number of different conditions.

In a specimen recently examined, interruption of dentine formation had occurred at some time and also resorption of part of the dentine had taken place from the pulp side, but there was the most unusual feature that the resorption cavities had been repaired with good tubular dentine.

The specimen was a lower deciduous canine tooth from a patient with cleidocranial dysostosis. It was removed at the age of 40, had never been decayed or filled, and was at that time still firm, the root being not much resorbed. Most of the pulp cavity had eventually become filled with dentine (Fig. 1). It can be seen that a temporary arrest of dentine formation occurred at a time when the tooth was almost complete, as indicated by a dark incremental line. On the side of this nearer the pulp the remains of the original pre-dentine can in many parts be detected. In the apical half of the root, however, resorption of the primary dentine has occurred in many parts, though this is quite superficial and has nowhere extended deeply into the primary dentine (Fig. 2). The formation of dentine has then recommenced equally at parts where the existing dentine had been resorbed and where it had not (Fig. 3). The first tissue deposited in the resorption cavities shows a few cellular inclusions (Fig. 4), but the terminal branches of the new tubules lie deep in the cavities and the tubules then extend to the present position of the pulp. Examination of an adjacent deciduous incisor and three permanent teeth showed no similar condition.

It seems likely that before resorption of dentine from the pulp side can begin there must be damage to the odontoblast layer and adjacent cells and possibly some calcification of the pre-dentine, and that recovery of odontoblasts rarely if ever takes place at that point, so that repair, when it occurs, is effected in a less highly differentiated manner. In the present case the position of the new dentinal tubules in the deepest parts of the resorption bays makes it probable that new odontoblasts were differentiated.

I had supposed the present observation to be unique but a single similar case was recorded by Warner *et al.* (1947, *J. Amer. dent. Ass.*, **34**, 469) though at an earlier stage of repair. In that case there was total pulpitis in an incisor with old silicate fillings, the resorption areas were again at the apical third of the root canal, and it was noted that signs of acute inflammation were present even in the region of repair.

Until further specimens are available it will not be possible to say what are the circumstances which allow this particular type of repair to occur following resorption or why it does not occur more often.

I am grateful to Mr. J. F. Dudley, A.I.M.L.T., for the histological preparations and to Mr. J. E. Hutchinson for the photomicrographs.

FIG. 1.—Lower deciduous canine tooth. × 4.

FIG. 2.—Section of root showing primary dentine (left), resorption and repair, and root canal centrally. × 36. Picrothionin.

FIG. 3.—New dentinal tubules passing from resorbed areas to pulp. × 70.

FIG. 4.—New dentinal tubules. × 180.

Section of Obstetrics and Gynæcology

President—GILBERT I. STRACHAN, M.D., F.R.C.P., F.R.C.S., F.R.C.O.G.

[*April 25, 1952*]

DISCUSSION: THE PRESENT CONCEPTION OF TRIAL LABOUR

Mr. W. Hawksworth: The last meeting on Trial of Labour in this Section took place on May 15, 1931 (*Proc. R. Soc. Med.*, **24**, 1521), when a number of Obstetricians presented the results from their various Hospitals. At that time there was still considerable debate as to whether cases of disproportion should be treated by induction of premature labour or by Trial of Labour. Since then I think it can be said there has been a complete swing away from induction of labour for disproportion in primiparous patients to Trial of Labour.

The problem then, as now, was to decide upon a definition of trial of labour. I decided for the sake of this paper to consider as a trial of labour one where at term there was evidence clinical, radiological, or preferably both, of cephalo-pelvic disproportion. This implies that the greatest diameter of the fœtal head cannot be made to pass through the plane of least dimension of the pelvic inlet by any method, including examination under anæsthesia, posture, or manœuvres such as that described by Munro Kerr; or that there is radiological evidence of disproportion.

There is considerable confusion of thought in the selection of cases for trial of labour. Too many people have considered the problem only in relation to the size of the pelvis—degrees of pelvic contraction—and seem to have lost sight of the fœtal head. In the 1931 discussion, the standard for a contracted pelvis in those cases presented from St. Mary's Hospital, Manchester, was a diagonal conjugate of 4″ to 4½″. No mention was made of disproportion between the head and the pelvis. Similar omissions were made in the reports from St. Thomas's Hospital, the General Lying-in Hospital, York Road, and Queen Charlotte's Hospital on that occasion.

It is true that what may appear disproportion to one observer may not be disproportion to another, but surely the answer lies in the management of the case and the ultimate result to mother and baby. If the fœtal head cannot be made to enter the maternal pelvis at term by using gravity and manipulative measures (with and without anæsthesia), and other causes of obstruction such as a full bladder and rectum, tumours, placenta prævia, etc., are excluded, then in my opinion disproportion exists and the patient is entitled to a trial of labour, unless of course the grossest contraction and grossest disproportion are present. The trial must naturally be an adequate trial without resort being taken early to the easy way out—i.e. Cæsarean section. This, of course, is the crux of the problem.

It is my opinion, therefore, that it is quite illogical to consider the pelvis without the baby when trial of labour is discussed. As Munro Kerr and Chassar Moir (1949) state: "Obstetric disproportion means an unfavourable relationship of the fœtal head to the maternal pelvis and it is illogical to study the one without the other."

It has been our policy in the Area Department of Obstetrics and Gynæcology at Oxford that practically all cases of cephalo-pelvic disproportion are given a trial. In this region, which drains cases over a wide area from five counties, we do not see perhaps the gross disproportions that are encountered in industrial areas of the North, but the pelves represent an average cross-section of those found in the Southern half of England. Moreover, we do have pregnant visitors from these less-favoured areas to add to the interest of our lives.

From the records of the Department we have extracted those cases in whom there was cephalo-pelvic disproportion, and who were treated as trials of labour during the years 1948–51. In these four years there were 122 patients submitted to a trial of labour—2 on two occasions—making 124 deliveries in all. Here are the results of the analyses of these cases and of other published series for comparison.

TABLE I.—RESULTS OF CASES TREATED BY TRIAL OF LABOUR

Hospital	Year	No. of cases	Mat. mortality	Mat. morbidity	Fœtal and neonat. mortal.	Fœtal and neonatal death			Spont. deliv.	Forceps deliv.	Cæs. S. after trial of labour	Elective Cæs. Sec.
						after spont. D.	after forceps	after Cæs. S.				
THE LONDON HOS-PITAL—Standard of contraction D.C. 3¼″—4¼″ D.C. above 4¼″ with clinical disproportion	At 1931 RSM Meeting For 1927, 1929, 1930	45	0%		8·8% 88 per 1,000							
ST. MARY'S HOS., MANCHESTER—Standard of contraction: D.C. 4–4¼″.	At 1931 RSM Meeting. For. prev. five years	96	2·2%		16·6% 166 per 1,000							
UNIVERSITY COLL. HOS., LONDON—Standard of contraction: True conjugate not more than 4″ or less 3¼″.	1932–1941	57	1·75%	12·3%	15·8% 158 per 1,000	13·6%	27·3%	8·7%	22 28·6%	11 19·3%	23 40·3%	
ROTUNDA HOS., DUBLIN—Clinical Report	Nov. 1949–Oct. 1950	156	1·2%		12·8% 128 per 1,000				110 70·5%		22 14·1%	24 15·4%
AREA DEPT. OBS. AND GYNÆ., OXFORD	1948–1951	124	0%	8·9%	1·6% 16 per 1,000	0%	0·8%	0·8%	29 58·9%	44	51 41·1%	0·8%

Table I.—The series from the London Hospital and St. Mary's Hospital were the only 2 that could be included from the 1931 discussion for they alone were the only ones which detailed trial of labour. The Rotunda Hospital cases were assessed in exactly the same way as ours, i.e. estimated presence and degree of disproportion by careful prenatal clinical and radiological examination.

TABLE II.—RELATIVE MORTALITY RATES

	Oxford Area Department				England and Wales		
Year	Total deliveries	Maternal deaths	Still-births	Neo-natal deaths	Maternal deaths	Still-births	Neo-natal deaths
1948	2,509	(3) 1·1 per 1,000 =·11%	16 per 1,000	17 per 1,000	0·73 per 1,000 =·073%	23·2 per 1,000	19·7 per 1,000
1949	2,638	(2) 0·75 per 1,000 =·075%	16 per 1,000	21 per 1,000	0·71 per 1,000 =·071%	22·7 per 1,000	19·3 per 1,000
1950	2,725	(1) 0·36 per 1,000 =·036%	15 per 1,000	13 per 1,000	0·69 per 1,000 =·069%	22·7 per 1,000	18·5 per 1,000
1951	2,814	(0) 0 per 1,000 =0% (6)	18 per 1,000	11 per 1,000		22·9 per 1,000	
Total	10,686	0·55 per 1,000	17·4 per 1,000	15·6 per 1,000			

TRIAL OF LABOUR (Present Series)

Years	Total No.	Maternal deaths	Still-births	Neo-natal deaths	
1948–1951	124	0%	8 per 1,000	8 per 1,000	124 Trials of Labour in 10,686 cases = an incidence of 1·1%

Table II.—The figures are *uncorrected*, as is the policy of our Department. The one stillbirth was a preventable death due to bad obstetrics—a high forceps delivery, and the one neonatal death that occurred on the third day after Cæsarean section was shown at autopsy to be due to a congenital biliary cirrhosis of the liver. It can be said that there was no fœtal death due to Cæsarean section.

The Cæsarean section rate is high, 41·1% (all lower segment) (and 40·3% in the U.C.H. series), whereas in the Rotunda series only 14·1% after trial of labour, but 15·4% elective section rate (against 0·82% in our series)—an overall rate of 29·5%. In the Report the Master (Prof. O'Donel Browne, 1950) writes: "The fœtal and maternal results suggest we have been too conservative in the use of lower segment Cæsarean section. Our fœtal and maternal results would undoubtedly have been better had we used section earlier and more fully in the Trial Labour group", and ... "our regrets are for instances of indecision and delay rather than any feeling of guilt in operating unnecessarily."

During this same period, 1948–1951, there were 263 Cæsarean sections for all conditions in the 10,686 cases, an incidence of 2·4% only. This, I think, compares favourably with the published

series of any hospital which serves a wide area and takes in all cases, emergencies and otherwise, and where no patient in labour is ever refused a bed.

It may well be argued that the Cæsarean section rate is too high in the Trial Labour series. Professor Nixon in his de Lee oration delivered at the Chicago Lying-in Hospital in October, 1950, made a plea for an extension of Cæsarean section in the treatment of prolonged labour. Many of the cases under review had a prolonged labour and as a result of our experience I find myself in complete agreement with Nixon (1951) when he states that much harm has resulted from the teaching that delivery must be at all costs *per vaginam* because the membranes have ruptured. As he says there is a place for the treatment of prolonged labour by Cæsarean section. This has been our departmental policy since the days when the Dept. was first founded and, in the years since, it is worth recording that 772 Cæsarean sections have been performed, including many grossly infected patients with no maternal deaths from sepsis. This is in no way a plea that treatment of trial of labour or prolonged labour when disproportion is present is by Cæsarean section. Far from it, our aim and object has always been to secure a vaginal delivery of a live child without harm to the mother.

In the present series, there were 73 cases who were in labour more than twenty-four hours, 27 more than forty-eight, 14 more than seventy-two, and 2 more than one hundred hours.

Of the 73 who were more than twenty-four hours in labour, 31 were delivered by Cæsarean section, 31 were delivered by forceps, and 11 were delivered spontaneously.

Thus it is seen that in those in whom labour was prolonged, there were more delivered vaginally than by section, 57% against 42%. Therein lies justification of the trial.

Prolonged Labour in a Trial

In our series 73 cases laboured for more than twenty-four hours, and of these 62 were delivered by operative means, either Cæsarean section, or forceps, with no maternal deaths, one stillbirth after forceps due to intracranial hæmorrhage, and one neonatal death after Cæsarean section due to a congenital abnormality. The closer attention to fluid balance, better analgesia, the introduction of antibiotics, greater use of blood transfusion and better anæsthesia have all played no small part in producing these results.

The use of antibiotics has made it possible to prolong the trial—with greater safety to the baby and to the mother—after rupture of the membranes. Many are loth to allow a labour to proceed longer than a few hours after the membranes have ruptured. In this series 67 patients had membranes ruptured more than twelve hours prior to delivery, 49 more than twenty-four, 23 more than forty-eight, 9 more than seventy-two hours, 3 more than ninety-six hours and 1 more than one hundred and twenty hours, with no loss of fœtal or maternal life attributable to sepsis. Thus the scope of a trial has been extended enormously by the judicious use of the chemotherapeutic agents now at our disposal. The morbidity rate (judged on B.M.A. standards of 100° on any two of the readings taken twice daily after twenty-four hours and before nineteen days) was: 8·9%.

Although I have detailed the number of hours some of the patients were in labour before they were successfully delivered, I do not advocate in any way that a trial of labour should be judged as adequate according to hours in labour. I would point out, however, that it is so easy to consider a trial as adequate when it has scarcely begun, or when the small hours of the morning are reached and the thought of a long day's work ahead prompts an already weary obstetrician to take the easy way out. Some criticism may be made in that in these series many patients were allowed to labour far too long before a decision was made to terminate the labour by Cæsarean section, but if decisions had been made earlier many more sections would have been performed and fewer patients would have had successful vaginal deliveries.

The ideal would be that all patients should come to full dilatation with membranes ruptured and adequate uterine forces present—and then the decision would be relatively easy. Though we may have pushed our labours rather far, only 58 of the 95 delivered by operative measures came to full dilatation, 43 of these 58 being delivered by forceps and 15 by Cæsarean section. Of the remaining 37 who did not reach full dilatation, 36 were delivered by Cæsarean section and one by forceps after incision of the cervix.

In a trial of labour there are several factors which may bring about a disordered uterine action, chief of these being the occipito-posterior position of the fœtal head. In this series there were 77 occipito-posteriors (confirmed radiologically and clinically) in the first stage of labour and of these, 27 had an inertia from the beginning of labour—a true primary uterine inertia—18 had fair contractions, later being adjudged as good, and only 32 had a good labour. The only index we have of the efficiency of the uterine action in the first stage is effacement and dilatation of the cervix. Of the 77 occipito-posterior positions at the commencement of labour, 32 had not reached full dilatation when labour was terminated. And at that time, 43 were still in an occipito-posterior position. Of the total number of Cæsarean sections in the whole series, i.e. 51 out of 124 cases, 35 Cæsarean sections (68·6%) were associated with an occipito-posterior position! This emphasizes the adverse effect of the posterior position.

The next major factor to be considered is the size of the baby. It is essential in considering trial

of labour to consider the baby—the size of the baby in relation to the maternal pelvis—for obviously the larger the baby the larger the pelvis must be for its safe passage. This can be stated in general terms only, for the position of the baby and the effectiveness of the uterine action can be determining factors in the outcome of a trial. In the series there are 2 patients who in their first pregnancies had successful forceps deliveries after trials, but who had larger babies in their second pregnancies and who both came to Cæsarean section.

A successful vaginal delivery on the one occasion does not necessarily herald a vaginal delivery on a subsequent occasion.

In the present series there were 36 babies who weighed over 8 lb. on the third day. Of these 36 only 10 were delivered spontaneously, 9 by forceps and 17 by Cæsarean section. It is the size of the head that counts. Mr. Derk Crichton has shown that the baby's head grows considerably in the last month of pregnancy and continues to grow when the pregnancy advances beyond the estimated date of delivery (E.D.D.), so there may still be disproportion at term even though the head enters the pelvis at the 36th week.

In our series there were 40 patients who came into labour more than eight days beyond the E.D.D. and of these 9 had spontaneous deliveries, and 31 (79·5%) required operative delivery (either forceps or section). This would suggest that the likelihood of delivery by forceps or by section increases with the duration of the pregnancy, and this must surely be attributable in part to the increase in size of the baby.

Size of Pelvis

It is wrong in my opinion to decide whether a patient should or should not have a trial of labour by consideration of the size of her pelvis alone. Whilst extracting the present series of cases from our records, I discarded not a few who had true conjugates of less than 10 cm., for the fœtal head in each case had been at the onset of labour in the pelvis, and hence there was no disproportion and a trial was unnecessary.

The size and shape of the pelvis

Many people in the past have used the diagonal conjugate as the measurement upon which to base a standard of contraction. This is not a measurement that can be made with absolute accuracy. Kaltreider (1951) has recently carried out a survey which shows the limitations of this measure.

Nowadays with the greater availability of X-ray machines and radiological examinations, the measurement of the true conjugate can be made with complete accuracy.

This measurement of the true conjugate in a lateral film of the pelvis is probably the standard index of contracted pelvis in use today. There is not, however, complete uniformity of opinion as to what measurement indicates contraction, 10 cm. or less is accepted by some, 11 cm. or less by others, and even 13 cm. by one international authority. This reliance on a standard index of contraction brings to light another fallacy. Disproportion of a degree that results eventually in delivery by Cæsarean section can occur in pelves where the true conjugate measurement is adequate and seemingly more than adequate—11 or even 12 cm.—due to a transverse contraction of the brim and the forepelvis, a point which has been emphasized by Professor Chassar Moir.

It would seem that transverse contraction of the pelvis is lost sight of for it receives no mention in so many Hospital reports of today. This may be due to the fact that in the Maternity Medical Report as drawn up by the College, there is no specific mention of transverse measurements under the heading of contracted pelvis. Nor is there any mention of cephalo-pelvic relationship.

Transverse measurements can be obtained only on supero-inferior views of the brim, and although I believe the lateral film, and by this I mean the erect lateral film, will provide almost all the information required of a pelvis, a supero-inferior view must be taken in those cases of disproportion where clinical examination of the pelvis has revealed a narrowing of the forepelvis.

TABLE III.—TRUE CONJUGATE IN CENTIMETRES AND TYPE OF DELIVERY

			8·5–9	9–9·5	9·5–10	10–10·5	10·5–11	11–11·5	11·5–12
Number of cases	..	..	4	10	32	28	27	14	4
					46				
					74				
Type of *delivery*:									
Spontaneous	..	..		3	9	9	3	5	
Forceps	..	..	2	3	9	14	7	6	1
Cæsarean section	..	..	2	4	14	5	17	3	3

Table III: Of the 119 whose films I have examined in detail and measured, 46 (38·65%) had a true conjugate measurement of 10 cm. or less, and 74 (62·18%) of 10·5 or less.

Midplane and outlet contractions do not, in my opinion, exist as separate entities. If they are present, then contraction of the upper pelvis is present as well, with one exception, the kyphotic pelvis.

The late Professor Marshall Allan, one time Assistant Master of the Rotunda Hospital and later Professor of Obstetrics and Gynæcology at the University of Melbourne, used to say that once

a fœtal head had gone into the pelvis it would come out below. He had not seen a midplane nor outlet contraction *per se* which had prevented a successful vaginal delivery. In this my colleague Mr. J. A. Stallworthy and I are in complete agreement. If midplane and outlet contraction are present to such a degree that successful vaginal delivery is impossible, then contraction is present in the upper pelvis to such an extent as to prevent the fœtal head entering the pelvic cavity. In the past five years over 13,000 deliveries have taken place in our department, and not one case has occurred to disprove this. We deplore the tendency in some circles to perform elective Cæsarean section in alleged contracted outlet.

What measures are there available by which we may attempt to predict the outcome of labour in a trial? There are, broadly speaking, two methods, clinical examination and radiological pelvimetry. In our department we use both, we believe in both. Is it possible by one method or the other or by both to predict the outcome of labour with 100% accuracy in these border-line cases that are submitted to trial? The answer is NO. If we keep this fact before us then perhaps we can view the problem dispassionately. With regard to radiological prediction of the outcome of labour in these cases there is no doubt of the value of intrapartum radiography to show the position of the fœtal head in relation to the brim of the pelvis. In a trial of labour it is often difficult to palpate and assess accurately the level of the fœtal head.

Kaltreider (1951), said "there seemed to be only one valid conclusion: in all pelves with a vertex presentation, without previous Cæsarean section, there must be a trial of labour" (in other words there is practically no place for elective Cæsarean section in the treatment of disproportion). "The handling of a suspect inlet contraction", he says, "is an art not a science." Further, "Actually the only possible thing in favour of the elective procedure is convenience to the obstetrician".

In August—a month before Kaltreider's independent publication in America—E. P. Allen and I (1951) wrote: "It appears to us there is no method of assessment of the inlet level which will allow a firm prediction to be given as to whether or not vaginal delivery will occur" and further "It is our opinion that, provided the presentation is a vertex and there are no other contra-indications, a full trial of labour is justified in practically every case of inlet contraction, and this can be embarked upon without prejudice to the mother or to the child".

Radiological Prediction of the Outcome of Labour

For any method to be absolutely successful it must be effective in the hands of all radiologists and obstetricians and not only in the hands of those who conceived it. It is by this and this alone that any method can hope to stand the test of time. As Kaltreider remarked after his investigation "it seems rather obvious from the results demonstrated that when the various criteria for inlet contraction are projected into use by another obstetrician their use is somewhat limited".

In assessing the value of prediction methods, too, we have to remember that what the obstetrician wants to know is whether vaginal delivery will be possible. It is difficult to forecast this when the baby's head has to be taken into account for cephalometry is such an uncertain procedure. Moreover, the higher the head, i.e. the further it is away from the brim, the more inaccurate does cephalometry become. And these are the cases, when the head-fitting test cannot be applied easily, in which the clinician requires help—and yet unfortunately the inaccuracy is greatest.

If this is true, then how can the outcome of labour be forecast consistently by radiological methods?

Finally, when predictions are made it is important to assess their accuracy not on the overall figures but on the border-line cases. The figures published by Williams and Phillips (1946) illustrate this point. They have an overall accuracy of over 90% correct predictions, but in the border-line (Group C) cases the prognosis was wrong in 13 of 24 assessable cases, i.e. *the error was over 50%*. There is also a 50% error reported by Williams and Arthure (1949). Even in expert hands there is a very considerable error in the prediction of border-line cases.

These points are emphasized not to decry the attempts that have been made to forecast the outcome of labour in a trial, but rather to stress the dangers that are inherent in this practice of relying on such predictions. These predictions are a great source of comfort to the obstetrician whose answer to all cases of dystocia is elective Cæsarean section. If he is unsure of his conduct of a trial or if he is not prepared to spend many weary long hours with a patient by day and by night, then he is delighted to have a radiologist's prediction which forecasts a degree of contraction or unshapeliness which he interprets as critical and so justifies his elective operation. Many series can be quoted to illustrate this point (Allen and Hawksworth, 1951). Pelvimetry in many hands undoubtedly increases the incidence of obstetrical interference and there lies, unfortunately, its greatest danger. It is our practice to use X-ray examinations freely but to keep them as ancillary aids and nothing more.

In conclusion I shall refer to Table IV. This is the analysis of the cases submitted to a trial of labour in our Department during the last four years.

TABLE IV.—AREA DEPARTMENT, OXFORD, 1948–1951
124 Cases

Spontaneous delivery	Forceps delivery	Cæsarean section	Maternal mortality	Maternal morbidity	Still-births	Neo-natal deaths
29 =23·4%	44 =35·5%	51 =41·1%	0%	8·9%	0·8%	0·8%

Table IV.—These results are those of a departmental team and show, I believe, the lengths to which trial of labour may be carried by those who are prepared to devote time and care to obtain good results.

In summing up, I would make a plea for:

A trial of labour in all cases of cephalopelvic disproportion, unless there is gross pelvic deformity.

An assessment of disproportion rather than a statement only of contracted pelves.

If contracted pelves are to be spoken of, then the transverse brim measurements should be stated as well as the true conjugate.

The trial should be continued preferably to full dilatation with membranes ruptured—providing the labour is advancing and there is no maternal nor fœtal distress. It is well to remember that as long as the cervix is dilating the labour is progressing. Descent may not take place until the membranes have ruptured, and frequently not until full dilatation is reached.

That the limitations of radiological pelvimetry be realized, and especially the radiological forecasts of the outcome of labour, and that we use them as they should be used—as ancillary aids to clinical judgment—and no more and no less.

That the safety limits in a trial be not judged entirely upon number of hours but by the maternal and fœtal condition both during and after labour.

Finally that by perfecting the art and practice of obstetrics we strive as always to achieve a live baby and a triumphant contented mother.

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Professor J. C. McClure Browne: Trial of labour is no new thing. In McClintock's edition of Smellie's Midwifery the following statement appears: "La Motte speaks of turning in difficult labours from disproportion, and recommends its performance after due trials of the powers of nature to force down the head." This presumably refers to his "Traité complet des accouchemens", Paris, 1721, as it is there that La Motte applies podalic version to head presentation.

Couvelaire, pupil of Pinard, was, I believe, the man who launched the phrase "trial of labour" in 1892, saying (and I translate freely) "Must we then conclude that one should await the trial of labour to make thereby an adequate pelvic assessment? Certainly not. For," he added, "operative interference in late labour is perilous, and medical art ought to be able to predict cephalo-pelvic disproportion before then".

But the trial of labour as we understand it today only came into being with the advent of reasonably safe Cæsarean section, so that we now have the alternatives of vaginal or abdominal delivery. It is quite clear that in most cases one may confidently predict safe vaginal delivery, while in some cases delivery of a healthy child can only be achieved by Cæsarean section. But between these two groups lies another, increasing in its scope, where we are uncertain of the issue, and this group of cases may be given the trial of labour.

In the decade of the First World War, Cæsarean section in labour still carried a high maternal mortality, and for this reason was resorted to comparatively rarely. Thus Campbell (1920) reported 92 cases of Cæsarean section performed late in labour with a maternal mortality of 15·5 per cent, and Holland (1921) a maternal mortality of 10·7 per cent. Bailey (1926), however, reported the results of a series of trials of labour in which lower segment Cæsarean section was performed in 38 cases, and his maternal mortality in these was nil, and fœtal mortality 4·2 per cent.

With the advent of the lower segment operation, blood transfusion, and antibiotics, and the concentration, because of the falling birth rate, on fœtal survival, Cæsarean section in labour has become commonplace. So much so, indeed, that few obstetricians in this country today have any wide experience of the complicated intra-vaginal and intra-uterine manœuvres whose descriptions fill the pages of the older editions of standard textbooks.

It was natural that at first the trial of labour should be applied only to cases of cephalopelvic disproportion, but since Browne (1922) drew attention to the frequency of intra-natal fœtal pneumonia where labour is slow there has been a gradually increasing awareness of the dangers brought to the fœtus by prolonged labour. Rudolph, in 1933, remarked the existence of two schools of thought when trial of labour was employed in contracted pelvis: the "anatomico-physiologic", where the trial involved the 2nd stage lasting for 2 hours with good pains, and the "clinical", where other

less harsh criteria were employed. He believed that in teaching undergraduates only the former definition should be employed, although he recognized that with increasing experience the "clinical" trial could be used.

The belief that the trial of labour should be employed only in cephalopelvic disproportion has been perpetuated, perhaps rightly, in the modern editions of standard textbooks. Thus in DeLee and Greenhill's "Principles and Practice of Obstetrics", 9th edition (1948) the test or trial of labour is referred to (p. 915) only in respect of disproportion. The same is true of "Operative Obstetrics" by Munro Kerr and Chassar Moir in the 1949 edition (p. 335).

In the last twenty years, however, it has become evident that the term "trial of labour" is being much more widely employed. For example, McLane (1930), writing on vaginal delivery following Cæsarean section, states that he sees no reason for subjecting certain selected patients to another section without a thorough trial of labour.

In conversation with Obstetricians from other centres both in this country and abroad I have been struck with the obvious acceptance of the term "trial of labour" in regard to many conditions in addition to disproportion where the course of labour cannot reasonably be foretold.

I have therefore attempted to find out what in fact is present practice in this respect, and to this end have obtained from 4 hospitals in this country—2 teaching and 2 non-teaching—details of cases which in the last five years have been given a "trial of labour". They are illuminating.

Thus in a total of 14,278 deliveries which took place in the 4 hospitals in five years, there were 566 cases designated as "trial of labour".

TABLE I

					Percentage
Total No. of trial labours	..	..	..	566	
Disproportion only	..	..	..	164	28·9
Disproportion + other factors	..	..	..	89	15·8
No disproportion ..	..	..	..	313	55·3

Of these, in only 164 cases was cephalopelvic disproportion the sole indication. *O tempora, O mores!* What then were the other indications? Broadly speaking, they fall into 6 groups. Table II shows these groups and their significance.

TABLE II

					Percentage
No disproportion	..	..	..	313	
Pre-eclamptic toxæmia	..	..	..	88	28·4
Inco-ordinate uterine action	..	..	..	51	16·3
Previous Cæsarean section	..	..	..	35	11·3
Elderly infertile	..	..	..	19	6·1
Others	..	..	..	46	14·7
Multiple indications	..	..	..	74	23·3

To this I would add another group—cases where surgical induction has been performed. I believe that one should never perform a surgical induction of labour (where the fœtus is of 34 weeks' maturity or more, and is normal) unless one is prepared to "back it up" with a Cæsarean section if delivery is not imminent at the end of forty-eight hours, because after this time the fœtal risk rises sharply to a serious level. The next table (Table III) is of interest, as it shows the benefits to the fœtus associated with the application of the forty-eight-hour rule. In the latter series, all cases conducted at Hammersmith Hospital, the inductions were largely for pre-eclamptic toxæmia, prolonged pregnancy, or unstable presentation.

TABLE III

Surgical inductions 4 hospitals 626	Fœtal loss (corrected) 17	Percentage fœtal loss 2·7
Surgical inductions "48-hour" rule applied (all Hammersmith cases) 91	Fœtal loss (corrected) 0	0

In this connection it is worthy of note that the "unripe" cervix does not contra-indicate the performance of surgical induction. A recent statistical survey by one of my assistants (Browne and Meagher, 1952) shows that there is no serious delay in the onset of labour in this condition, when compared with that with a "ripe" cervix.

I believe that the restriction of the use of trial of labour to cephalopelvic disproportion is outdated, and that it should be, and is being, used in any cases in which special supervision should be exercised, and the labour conducted in an institution where operative interference can be initiated in good time and in proper surroundings.

Let us see what are the results of such a system.

TABLE IV

						Maternal mortality (corrected)
14,278 deliveries	..	..	..	..	..	0·86/1,000
566 trials of labour	..	..	..	..	..	0

In the period covered by this survey, a total of 14,278 deliveries occurred in the 4 hospitals. If we accept for this series the Registrar-General's 1948 figure for maternal mortality of 0·86/1,000 (corrected) we can compare it with the 566 cases of trial of labour in which the maternal mortality was nil. Of course many of the deaths occurring in the total series were due to intercurrent disease, etc., and these conditions prevented the use of trial of labour, in which group none of them appear.

The stillbirth and neonatal death rate (Table V) was 48 per 1,000 in the whole series, and 24·7 per 1,000 in the cases of trial of labour.

TABLE V

					Per thousand
Fœtal loss in whole series of 14,278 ..	..	..	..	..	48
Corrected for prematurity ..	..	..	..	..	24
Fœtal loss in 566 trials of labour	·.	..	..	..	24·7
Fœtal loss in 301 trials at Hammersmith	..	..	..	..	15·5

If we allow that 50% of the mortality in the whole series was due to prematurity, we find that the corrected stillbirth and neonatal death rate is roughly equalled by that in the cases of trial of labour—and these were all cases in which possible difficulty was foreseen.

The Cæsarean section rate in the whole series was 4·5% and in the cases of trial of labour 22·8%, which is lower than might have been expected. Vivian Barnett (1942) found a section rate of 40·3%, though in all his 57 cases disproportion was present. At Hammersmith Hospital in the period under survey there were 69 cases of contracted pelvis treated by trial of labour. There were 28 Cæsarean sections, an incidence of 40·1%, and two babies were lost, a fœtal wastage rate of 2·9% for the total series compared with that in Barnett's series (15·8%).

It is obvious that as our knowledge of the art of obstetrics advances, new indications for the trial of labour will arise. I can hear someone saying that soon every child not born by elective section or "B.B.A." will arrive as the end-result of a trial of labour. But this will never be so, for advancing knowledge will not only bring new indications, but will also invalidate some old ones such as pre-eclamptic toxæmia or inertia.

I would draw your attention to an interesting feature of many of these cases of trial of labour, the "double hazard". By this I mean that the patient started her labour with two "handicaps", as Mr. Aleck Bourne calls them, each of which in itself did not constitute a clear indication for trial of labour or Cæsarean section, but the sum of the two indicated special care in the conduct of labour.

TABLE VI

Double hazards ..	..	..	..	163	
Cæsarean sections	..	..	..	58	35·6%
Fœtal loss	..	..	..	6	36 per 1,000

It will be seen that the Cæsarean section rate and fœtal loss are significantly higher (35·6% and 36/1,000) in this group, but the remainder had a vaginal delivery and a live baby. Examples of the double hazard are the elderly infertile woman with mild pre-eclamptic toxæmia, and slight disproportion with mild pre-eclampsia.

When trial of labour is performed for disproportion I would suggest that this be called a *cephalo-pelvic test*, but grouped with all the other cases under the general term "trial of labour". This wide application of the latter term conveys to all concerned with the case the necessity for close supervision, whatever the indication.

There is no doubt that Couvelaire (loc. cit.) was right when he referred to the dangers of operative interference (he used the word "improvisation") late in labour. Today, thanks to earlier interference, the antibiotics, transfusion, and improved anæsthesia, the danger to the mother is reduced nearly to negligible proportions, but the danger to the fœtus from asphyxia and infection is still great, as shown recently by Eastman. The label "trial of labour" increases the chance of operative interference being undertaken in good time and in proper circumstances.

I have attempted to show, then, that the trial of labour is widely, and wisely, practised today for many reasons other than that of cephalopelvic disproportion; that its results fully justify its employment; and that the indications for its use are likely to expand.

I would end with a plea to the Royal College that among the many headings in its model maternity report some nook be found for "trial of labour" in its own right, and not simply as an appendage to "Contracted Pelvis".

Finally I must acknowledge my gratitude to Professor Nixon, Mr. Philip Mitchell, Mr. James Mair, Mr. John Gibson, and Mr. Fisher and Mr. Meagher of my own staff for the case details used in this paper.

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The Accuracy of X-ray Cephalometry *in Utero*

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The primary concern of this paper[1] is the measurement of the size of the fœtal head with particular reference to the diagnosis of disproportion, and the author's observations are also directed towards the main subject under consideration by the meeting, namely "The present conception of the trial of labour".

Radiological cephalopelvimetry is frequently performed before term and appropriate allowance must therefore be made for the subsequent growth of the fœtal head in order to appreciate the cephalopelvic relationship which will exist at term. The average growth rate of the bi-parietal diameter of 34 cases in the present investigation was found to be 1 millimetre per week. It follows from this slow growth rate that a head which will be large at term is already moderately big by the 37th week of pregnancy. A particularly interesting finding in a further 9 cases was that a similar growth rate continues when pregnancy progresses two to three weeks beyond term.

It is also necessary to consider the consequences of flexion and moulding of the fœtal head, because it is by producing these changes that uterine action is sometimes capable of effecting a vaginal delivery despite the presence of seemingly insuperable disproportion. Thus if we consider the average head of a 7½ lb. baby, the area of head engagement measures 87 sq. cm. if the head is deflexed, but if it flexes the area is reduced to 72 sq. cm. Furthermore, whereas average moulding of a deflexed head only reduces the presenting area to 82 sq. cm., average moulding of a flexed head reduces the presenting area to 65 sq. cm. Consequently, the total reduction of the presenting area of a deflexed head which is produced by flexion and average moulding is about 25%.

For this reason it is important to realize that even if cephalometry were to be completely accurate in establishing the antepartum size of the fœtal head, this size may be greatly exaggerated or reduced by many factors during labour, and corresponding alterations in cephalopelvic ratio will result.

Many radiological methods of cephalometry may be employed to help determine whether or not a fœtal head will pass through a particular pelvis. These methods rely, either upon measurement of the length of a diameter of the skull, or upon measurement of its average cranial circumference (which is indicative of its volume), and the reliance which is placed upon the accuracy of the results obtained varies with different observers.

If the cranial measurements are compared of babies weighing 5½ and 10½ lb., the average cranial circumference (as measured with a map measure) has the greatest "practical range", namely 5·5 cm. The method of cephalometry also possesses the advantage that head measurement can be made in all positions of the head. It is unfortunate, however, that this method of cephalometry is not a precision-

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[1]This paper was based upon clinical material examined in the Nuffield Department and the Area Department of Obstetrics and Gynæcology, Oxford.

The radiology and the majority of the radiography was undertaken personally. Two hundred and fourteen patients, who had been referred with a clinical diagnosis of disproportion, were examined by Moir's (1949) method of cephalopelvimetry. An additional application was made of Ball's (1935) principles of cephalometry (whereby the average cranial circumference is measured). The accuracy of antenatal cephalometry was checked by radiological post-natal cephalometry in 170 babies. Further radiographs designed to assess cranial moulding were taken of 90 of these babies.

method, for it is designed to give a general impression of the size of the head. The error in cephalometry fell within the range of 1 cm. in 40 checked cases examined by this method in the present investigation. Yet the author has had experience of a further case,[1] in which the method of check was not entirely accurate, but in which the error in cephalometry must have been almost 1·5 cm.

If the same comparison is made in the case of the bi-parietal diameter, the "practical range" is found to be only 2·0 cm. but this 2 cm. represents a difference of 5 lb. in weight—the difference between a small (5½-lb. baby) and a large (10½-lb. baby). Consequently, an error of a few millimetres in measurement of the bi-parietal diameter may lead to serious errors.

Whereas it must be admitted that the diameter of the fœtal head which possesses greatest practical significance is the bi-parietal diameter, a further disadvantage of relying entirely upon this measurement exists in the fact that the diameter "appears" larger when the skull is rotated from the true anteroposterior view. According to the author's findings the length of the bi-parietal diameter is usually not affected until the skull has rotated 10 degrees, and an "exact" measurement can then be made. When skull rotation amounts to 30 degrees, however, the "apparent" length already exceeds the true length by 0·6 cm., and it is therefore impracticable to rely on the accuracy of the measurement, because the magnitude of error is excessive in relation to the small "practical range" of this diameter, which is only 2 cm.

Moir (1949) has pointed out that the measurement of the bi-parietal diameter in unfavourable skull positions may be helpful in a negative sense, in that it enables one to state that the head is smaller than the radiological measurement suggests. On the other hand, it is necessary to stress that the temptation is real and that it is dangerous to employ this measurement in the positive diagnosis of disproportion.

It is obviously desirable to appreciate the frequency with which the skull assumes an unfavourable position in practice. If the head did not have a predilection for any position, it would be impracticable to measure the bi-parietal diameter in 66% of cases, but in 214 cases examined in the present series it was impracticable to measure the bi-parietal diameter in 33% of cases.

Unfortunately the type of case in which cephalopelvimetry is desirable is the very case in which the head is high, not nearly engaged in the pelvic brim, and in an unfavourable position. This renders exact measurement of the bi-parietal diameter dangerous, if the information is employed to make a positive diagnosis of disproportion.

When the head lies in an unfavourable position it is common practice to deduce the length of the bi-parietal diameter from the known length of the suboccipitobregmatic diameter, Reece's[2] diameter, or the occipito-frontal diameter. Moir (1949) and Heyns (1945) have already shown that this deduction from the suboccipitobregmatic diameter is unsafe in normal babies, and in the present investigation among cases of disproportion Reece's diameter differed from the bi-parietal diameter by 0·4–0·7 cm. in 25·3% of cases, and by as much as 0·8–1·0 cm. in 7% of cases. The deduction of the length of the bi-parietal diameter from the known length of the occipito-frontal diameter by Thoms' (1930) method was equally inaccurate, because the deduction was incorrect by 0·2–0·5 cm. in 47% of cases, and the deduction was incorrect by 0·6–1·0 cm. in no less than 23% of cases.

Were the widely held view correct that fœtal skulls are moderately uniform in shape, the relationship between the various cranial diameters would be constant. It is obvious, however, from the findings of this investigation, that a disturbingly high variation in head shape exists among those very cases in which cephalometry would be most desirable. Fig. 1 shows two unmoulded heads which possess the same volume.

The following explanation is offered for this phenomenon of "shaping". Firstly, the direction of growth of the head tends to be minimal in the path of greatest resistance, and secondly, the head is also gradually "shaped" by abnormal pressure effects which it may encounter. This process, which the author has termed "shaping", differs from the far greater distortion caused by moulding, mainly in that moulding is obvious, rapid in development, and soon disappears; whereas "shaping" entails less distortion, is insidious in onset, is slow to disappear, and is often overlooked by the observer.

The phenomenon is likely to occur after the 37th week of pregnancy in cases of disproportion and is particularly common in breech presentation. Its presence will lead to serious errors in estimating the size of the fœtal head, if complete reliance is placed upon accurate measurement of a single diameter of the fœtal head.

Table I illustrates some important obstetrical aspects of cephalopelvimetry in disproportion. In the 14 patients described, a major degree of cephalopelvic disproportion as diagnosed at the pelvic inlet by various radiological methods. Despite the unfavourable measurements which are listed, and despite the adverse radiological forecast which would have persuaded many obstetricians to

[1] This error in cephalometry was caused by excessive movement of the fœtal head between two successive radiographs.

[2] Reece (1935) assumes that the fœtal skull is oval, or egg-shaped, and consequently states that the smallest diameter of the largest cross section of the skull is equal to the bi-parietal diameter.

TABLE I.—FOURTEEN CASES IN WHICH A RADIOLOGICAL FORECAST OF A MAJOR DEGREE OF CEPHALOPELVIC DISPROPORTION WAS MADE BY MOIR'S, AND OTHER RADIOLOGICAL METHODS

Para.	Weight pounds	Bi-parietal diameter cm.	Average cranial circum- ference cm.	True conjugate cm.	Available transverse of inlet cm.	Shape of sacrum	Dilatation of cervix	Mode of delivery
2	7·14	8·8	32·2	8·8	14·2	Normal	¾	L.S.C.S.
0	6·15	9·2	31·4	9·2	11·8	Str.	Full	L.S.C.S.
0	7·13	9·8	33·4	9·6	12·1	Str.	Full	L.S.C.S.
4	10·1	10·2	35·2	10·5	12·2	Normal	¾	L.S.C.S.
2	7·14	9·6	32·8	9·4	12·0	Normal	Full	Forceps
0	7·7	9·5	32·5	9·5	12·1	Normal	Full	Forceps
0	7·11	9·5	32·8	10·1	11·1	Str.	Full	Forceps
2	7·2	9·4	32·4	9·5	11·3	Normal	Full	Forceps
0	6·12	9·5	31·9	9·7	11·8	Str.	Full	Forceps
0	7·5	9·3	32·5	10·5	10·2	Str.	Full	Spontaneous
2	10·0	10·2	35·2	10·0	12·2	Normal	Full	Spontaneous
1	8·1	10·3	34·8	10·3	12·0	Normal	Full	Spontaneous
2	7·1	9·5	32·8	10·7	10·8	Str.	Full	Spontaneous
1	7·2	9·9	32·8	10·0	11·6	Str.	Full	Spontaneous

prefer an elective Cæsarean section to a trial of labour, no less than two-thirds of these patients were delivered vaginally with safety to mother and baby.

It should be noted that the error in radiological forecast emanated from shaping of the head in some instances, as illustrated by the disproportionately small or broad bi-parietal diameter measurements, which are in italics. In some of these cases, the additional measurement of the average cranial circumference may have helped to avoid these errors.
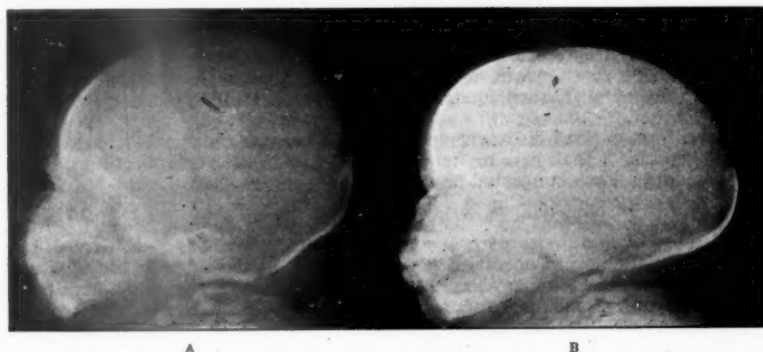

FIG. 1.—A comparison of the shape of the head of a vertex baby (A), and a breech baby (B), weighing 7 lb. 9 oz. and 7 lb. 6 oz. respectively. Whereas the volume of the skulls was the same, the occipito-frontal diameters differed by 0·6 cm. (11·5 : 12·1 cm.), and the bi-parietal diameters differed by 0·5 cm. (9·4 : 9·9 cm.).

Summary and conclusions.—It is my belief that where absolute significance is attached to accurate measurement of a single diameter of the fœtal head, grave mistakes will be the penalty, questionable gains the advantage. Gross errors will be partly avoided in some instances by checking diameter measurements with measurements of the average cranial circumference.

It is not possible to achieve consistent accuracy in cephalometry; consequently, it is not possible to achieve consistent accuracy in cephalopelvimetry. For this reason, and because of the major reduction in head size which may be produced by flexion and moulding, without considering further unpredictable factors which may operate during labour, absolute reliance cannot be placed upon a positive radiological diagnosis of disproportion.

The author is indebted to Professor J. Chassar Moir and Mr. J. A. Stallworthy for their advice, and for the facilities for conducting this investigation.

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The following specimens were shown:

(1) **Endometriosis of Pleura and Ovaries.** (2) **Tetanus Following Colpoperineorrhaphy.**—Miss JOSEPHINE BARNES.

Acute Procidentia.—Mr. ARTHUR BELL.

An Unusual Fundal Fibroid.—Mr. S. BURKE.

Carcinoma of Fallopian Tube.—Mr. MALCOLM DONALDSON.

Utero-adnexal Infarction in Pregnancy.—Dr. J. ELSTUB.

Stromatous Endometriosis.—Dr. M. P. EMBREY.

Transtubal Spread of Carcinoma Corporis Uteri.—Dr. H. P. FERREIRA.

Struma Ovarii Containing a Piece of Thymus.—Dr. P. E. HUGHESDON.

Para Ovarian Carcinoma.—Dr. D. J. MEAGHER.

Primary Ovarian Pregnancy.—Dr. K. READ.

Carcinoma of Urethra.—Mr. T. F. REDMAN.

Mixed Mesodermal Tumour of the Cervix Uteri.—Dr. C. W. TAYLOR.

Mesenchymoma of Broad Ligament.—Mr. S. W. WRIGHT.

[*June* 27 1952]

Valvotomy in Pregnancy

By R. C. BROCK, M.S., F.R.C.S.

THE successful establishment of cardiac surgery has affected the prognosis and treatment of certain heart diseases so that it now touches closely upon the speciality of obstetrics. I must emphasize that in some ways my communication is premature and I would have preferred to have waited longer before presenting it, but I agreed to do so because I felt the importance of the changed situation justified it. Although our experience in valvotomy during pregnancy is as yet small, it is large enough and significant enough and of such fundamental importance that the sooner the information is disseminated the better.

The commonest form of heart disease complicating pregnancy is rheumatic, and the most frequent lesion is mitral stenosis. I shall base my remarks largely upon the problem of mitral stenosis, but I shall say a few things about congenital heart lesions.·

THE PROBLEM OF MITRAL STENOSIS

The surgical relief of mitral stenosis was pursued, almost as the will-o'-the-wisp of surgery, for many years; the first attempts at operation were made soon after the first World War and were so unsuccessful that in the minds of many the issue had been decided and operation had been shown to be impossibly dangerous. Some four years ago new efforts were made to solve the problem both in this country (Baker *et al.*, 1950) and in the United States (Bailey, Glover and O'Neill, 1950; Harken *et al.*, 1948; Smithy, 1949). These efforts were rewarded by success and the teams in which I work at Guy's and the Brompton Hospital have recently recorded the results in our first 100 cases (Baker *et al.*, 1952).

There were 13 deaths, but 7 of these occurred in the first 20 cases, only 6 in the last 80. The results have been good or excellent in three-quarters of the survivors in the first 50. The results since the first 50 have been even better.

The success of the operation is shown by its wide acceptance and practice and we now know that although mitral valvotomy was thought for so long to be an impossibly dangerous procedure, it is now firmly established as a standard surgical intervention with a low mortality and good results.

As a result of this success a huge new problem has been revealed in that very large numbers of patients are being presented or are presenting themselves for operation. The numbers are so large that it is quite impossible to cope with them unless a service is provided in each large centre throughout the country. At my own two hospitals it is not possible to accept any but a small proportion of those seeking help. It used to be said that the number of patients needing mitral valvotomy was few; this has now been shown to be totally wrong.

The size of the problem can be appreciated when I tell you that it has been estimated that there are some 250,000 cases of mitral stenosis in Great Britain. This means roughly 5,000 per million of population. I do not know what proportion of these patients need or will need operation, but it is clear that even if the proportion is low it is going to be very difficult to deal with the numbers, seeing that there are so many cases per million of population.

As you know the disease is especially common in young women of child-bearing age.

MITRAL STENOSIS AND PREGNANCY

Many women with mitral stenosis go through pregnancy and delivery without obvious harm. On the other hand many develop serious trouble; they may go into heart failure or may die. Many patients date their serious deterioration and subsequent progressive invalidism from a pregnancy. Morgan Jones (1951) in his recent book "Heart Disease and Pregnancy" points out that three factors increase the liability to failure; these are augmentation of the cardiac output, increase in the blood volume, and retention of salt and water. The most important cause of serious symptoms in pregnancy is pulmonary congestion leading to severe and even fatal attacks of pulmonary œdema.

The common practice up to now, and indeed even still, is to terminate the pregnancy in many cases and often at the same time to sterilize the patient.

In the past this may well have been a justifiable practice. It may be that even to-day it is still correct in some cases. I would suggest, however, that this policy is no longer justifiable in most cases, and at any rate usually cannot be justified until the valve is actually examined at operation.

No one observing large numbers of these patients can fail to observe the frequency with which the marriage is broken up. It seems that it often fails to survive the double strain of the wife's disability and her sterility. The problem, therefore, touches upon more than the sacrifice of one unborn child. It prevents other children being born and may cause the break-up of the home.

I am appalled by the frequency with which one sees young women in the early twenties who have not only suffered termination of their pregnancy but have been sterilized as well. Quite a number of these young women come for operation simply and solely because they want children. It is indeed a tragedy if they have already been prevented from conceiving again.

This situation has been entirely changed by the advent of mitral valvotomy, especially now that we know the operation can be safely done during pregnancy and the child allowed to live; the mother delivering herself naturally and without danger and, moreover, being able to face future pregnancies with confidence.

Pregnancy of course enforces an extra strain on a woman with heart disease and her life may be placed in peril. Severe pulmonary congestion, large hæmoptyses or attacks of pulmonary œdema are especially liable to develop.

Instead of terminating the pregnancy, or guiding the patient perilously through her many months of extra strain culminating in the final problems and perils of delivery, the whole problem can be settled by valvotomy.

Even if pregnancy and delivery are successfully surmounted without operative help the patient is often left with a severe increase of symptoms and disability which marks the beginning of a steady deterioration.

Valvotomy during pregnancy allows safe continuance with avoidance of further strain on the heart and, moreover, after delivery the mother can look after her child, her home and her family instead of being an invalid for many months or years.

I have stated that our experience is as yet small but we have, so far, operated on 3 patients during pregnancy, all of whom were having severe symptoms of a pulmonary congestive type and all 3 have done very well. The pregnancies continued without further event and in each case natural delivery occurred without trouble or anxiety. One young woman operated on during the third month was a primipara and arrangements had already been made elsewhere to terminate the pregnancy and sterilize; a second was operated on at the 28th week of her second pregnancy; the third during the 28th week of her fifth pregnancy.

This last patient introduces another aspect of the problem; namely the moral or religious side. She had had one pregnancy terminated on account of severe symptoms but had been forbidden by her priest to accept termination of the next pregnancy. I have been told that in Catholic communities in which termination is forbidden, little harm results and the pregnancy is well borne. This is not our experience and was certainly not the case with this patient whose condition was critical and it was clear that she was unlikely to survive to the end of the pregnancy.

At operation she was found to have a very small mitral orifice but the cusps, although adherent, were still thin and supple and were divided with ease; an almost normal-sized opening with good functioning cusps resulted. The clinical improvement has been commensurate.

I wish to emphasize that it is difficult or impossible in any one case to make a complete and final assessment of the prognosis until the valve is actually examined at operation. It may well be that in a particular case the patient may have such severe multiple valve lesions with a huge heart and perhaps long-standing or severe failure that it is clear the pregnancy should not be allowed to continue and further pregnancies should be prohibited.

Alternatively at operation the valve may be found to be so damaged, so rigid, fibrous or calcified, that it is impossible to improve its function much. Termination may then be indicated and justified, and sterilization also. On the other hand it is often quite impossible to make this assessment without operation and in an apparently identical case the valve is found to be readily amenable to complete surgical division and the immediate and later prognosis can be confidently assessed as good.

Modern cardiology is not limited to clinical, radiological and laboratory examination. It extends into the operating theatre where the final, more complete and more exact assessment can often be made. It is important that we should know and appreciate this.

CONGENITAL HEART DISEASE

This group is less numerous but nevertheless often presents difficult problems and it should be a great help if it is known that operation (not necessarily upon the heart itself) can safely be done during pregnancy.

Persistent ductus arteriosus may cause actual or threatened failure and there is no reason in such cases why the ductus should not be closed and the patient spared the great extra mechanical burden she carries from the arteriovenous leak.

Coarctation of the aorta is also uncommon in pregnancy but constitutes a great extra risk and Morgan Jones mentions the danger of rupture of the aorta or of cerebral hæmorrhage during labour and states that of 6 reported deaths during pregnancy in 53 patients with coarctation, 4 were due to vascular accidents. Operation should clearly be undertaken to correct this risk if the severity of the disease indicates the need.

In the congenital cyanotic group pregnancy is unusual, I have had one patient aged 27 who had been married seven years without becoming pregnant, and who was very blue and disabled from a pulmonary valvular stenosis with a reversed flow through a patent foramen ovale. Shortly after discharge from hospital after a successful pulmonary valvotomy, she became pregnant and as she was so well the pregnancy was allowed to continue; she delivered herself naturally eleven months and two weeks after operation. This was nearly four years ago and she has been able to look after the baby and her family without any disability at all.

One year ago Dr. Paul Wood asked me to perform pulmonary valvotomy on a woman aged 26 with a pulmonary valvular stenosis with closed septa and who was five months pregnant. This was done and the pregnancy proceeded to normal and safe delivery and she too has been able to look after her child without trouble.

It appears, therefore, even in spite of our relatively small and short experience, that in the present ability to perform successful heart operations during pregnancy with low mortality and with good results we have a very important addition to what we can offer to our patients. Close co-operation is needed between cardiologist, obstetrician and surgeon if the best results are to be obtained. The cardiologist and the cardiac surgeon, are, however, in the hands of the obstetrician in this matter for they may well not see the patients at the time of their pregnancy with a view to operation unless the obstetrician refers them. How recently the position has changed is illustrated by the fact that in Morgan Jones' book on "Heart Disease in Pregnancy" published in 1951, no mention whatsoever is made of the possibility of operation in the management of these difficult cases. Clearly, therefore, this changed position in regard to pregnancy and heart disease that has resulted from the advent of successful cardiac surgery should be made widely known.

The paper was illustrated by a film.

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Uterine Contractility in the Third Stage of Labour [*Abstract*]

By Professor H. ALVAREZ and Professor R. CALDEYRO-BARCIA (*Montevideo, Uruguay*)

AFTER the birth of the child, the umbilical vein is connected with a mercury manometer, which records the pressure exerted on the placenta by the contractions of the upper segment during the third stage of labour.

These contractions continue those of the second stage without any interruption. Their average intensity (50 mm.Hg) and frequency (4 per ten minutes) are similar to those of the second stage, but they are absolutely painless (Alvarez and Caldeyro-Barcia, 1950).

These contractions separate the placenta and expel it to the birth canal. They also drive blood from the placenta to the child.

The upper segment time (U.S.T.) is measured from the birth of the child to the moment in which the placenta is expelled from the upper segment. Its average value in normal cases is six minutes.

Between ten and thirty minutes is Prolonged U.S.T., and more than thirty minutes is Retention of the Placenta. In the 80 cases recorded there are 13 Prolonged U.S.T. and 5 Retention of the Placenta. In these cases the contractions have normal intensity and frequency but abnormal co-ordination.

Ergometrine, methylergometrine and pituitrin increase the tonus and frequency of the contractions and reduce the U.S.T.

Only pituitrin reduces the proportion of Prolonged U.S.T. and Retention of Placenta.

Analgesia with trilene during the second stage, increases the proportion of Prolonged U.S.T. and of Placental Retention.

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Section of Physical Medicine

President—H. F. TURNEY, D.M., M.R.C.P., M.R.C.S.

[*January 9, 1952*]

Conservative Treatment of the Lumbar Disc Syndrome [*Abridged*]

By E. J. CRISP, M.B.

THE lumbar disc lesion knows no age limit, affecting most of us sooner or later, to a greater or lesser degree. But in spite of the fact that the disc is comparatively avascular, it is a lesion which tends to heal spontaneously under favourable conditions. Coventry, Ghormlie and Kernohan (1945) in an investigation on a series of lumbosacral discs removed from cadavers of varying ages observed vascularization of annular fissions and tears. This they regarded as an attempt by nature to nourish and repair a damaged structure. More recently, Smith and Walmsley (1951) have shown that if the ventral aspect of a rabbit's lumbar disc is incised, the incision reaching the nucleus, repair of the more superficial annular fibres takes place in three weeks. Apart from this experimental evidence, sufficient cases treated by conservative methods five or more years ago have now been followed up to show that disc lesions can and do recover completely when treated conservatively. Repair presumably takes place by vascularization of the tear by vessels from the spongiosa or by extradural vessels, with the resultant formation of scar tissue.

It is essential to understand what is meant by the term "disc lesion", for expressions such as "slipped disc" or "prolapsed disc" are most misleading. The lesion as a rule consists of a small postero-lateral annular bulge, the result of a partial splitting of the fibrocartilage. It is only when the tear is complete that actual protrusion of the nucleus takes place.

The lesion may be compared with the bulge which occurs in the wall of a motor tyre after some of the fibres in the wall of the tyre have ruptured. As more and more fibres rupture the bulge becomes bigger and bigger until finally a complete rent appears through which the inner tube starts protruding.

Recurrent attacks of acute low back pain are indicative of recurrent ruptures of annular fibres. With each successive annular rupture there will be an inflammatory reaction and possibly local œdema, and it must be borne in mind that lumbago and sciatica are symptomatic of the same lesion, crural pain only occurring when the annular bulge is so situated as to stretch or irritate a nerve root.

Some extent of the lesion can be gauged from the clinical picture. Thus with a small annular bulge, the lumbar spine is usually held rigidly lordosed by protective muscle spasm, little or no forward flexion being possible. Conversely, with a large protrusion the lumbar curve is reversed and though forward flexion may be possible, the patient is unable to stand upright. In both cases the posture is protective.

Early diagnosis is an essential part of treatment. The differential diagnosis is clear cut, and it is important to steer clear of such diagnoses as sacro-iliac strain or fibrositis and to learn to distinguish between protective and faulty posture.

The object of our treatment is to allow any bulge or protrusion to reduce and, following this, repair to take place. Among the methods at our disposal are bed rest, plaster jacket, traction, manipulation, epidural injection, physiotherapy and rehabilitation, spinal support and resettlement.

AUG.—PHYS. MED. 1

Bed rest alone usually suffices to relieve the average attack of acute lumbago completely, but in the severer type of case in which the symptoms are more persistent it not infrequently fails. This is usually the result of nature's calls, the trip to the toilet or the use of the bed pan interfering with attempts to keep the spine immobile. Bed boards should of course be employed if the mattress is not reasonably firm.

The plaster jacket remains our most effective measure for the conservative treatment of disc lesions. It has many advantages over best rest for it splints the back, produces rapid relief of pain and, by allowing the patient to become ambulatory at a comparatively early date, is excellent for the morale. Certain rules must be observed if the jacket is to be successful. No attempt should be made to correct the patient's posture, for it is protective; the jacket must be tight and accurately moulded to the patient's body and must be worn for at least four weeks after all pain has disappeared, in order to allow sound repair to take place. These rules apply to the case with the reversed lumbar curve, the plaster being applied with the lumbar spine kyphosed. Usually the protrusion reduces in the course of a few days and the normal lumbar curve returns. The first plaster is then removed and a further one applied with the lumbar spine in its normal posture. All cases should be supplied with a lumbar brace when the plaster is finally removed. This should be worn for upwards of six months and occasionally longer.

Traction.—Though I have long recognized traction as the only really safe method of manipulating an acute lumbar disc lesion (Crisp, 1949), until comparatively recently I only used manual traction. In the past twelve months, however, I have employed sustained traction on a simple traction table of my own design, in the treatment of acute and chronic lumbar disc lesions.

The case with the reversed lumbar curve can often be relieved dramatically after two or three treatments, the protrusion reducing rapidly and the normal lumbar curve returning. A plaster jacket should then be applied to prevent recurrence and allow the lesion to heal.

If the lumbar spine is held tightly lordosed by protective spasm, traction is less effective, but is at least unlikely to do harm. Possibly 50% of cases of this type with or without sciatica will be relieved to a greater or lesser extent.

Traction is also effective for the relief of residual pain following prolonged bed rest or immobilization in plaster, and also sometimes for the relief of recurrent attacks.

Manipulation, excluding traction, is in my opinion contra-indicated as treatment for an acute disc lesion. For the relief of symptoms in the chronic stage of the condition, or persistent pain after the lesion may be presumed to have healed, gentle repeated manipulations are often extremely effective. These should be performed at intervals of a few days and without anæsthesia.

Physiotherapy is, in one respect, a mixed blessing so far as disc lesions are concerned. Many patients who would do extremely well with bed rest alone feel that they are being neglected unless they attend hospital for physiotherapy. The combination of bed rest and physiotherapy often prolongs rather than cures the condition, because the visit to hospital cancels out the benefit of rest.

For the chronic case short-wave diathermy is of considerable value both for the relief of backache and sciatic pain. Hyperextension exercises are invaluable and should be practised by all lumbar disc cases. Exercises to increase lumbar flexion should be absolutely forbidden, this movement being left to return spontaneously. In those cases in which lumbar flexion remains grossly restricted, a compensating increase in hip flexion often takes place.

Patients should be taught how to lift heavy objects by bending the knees and getting under the weight, and also taught what actions to avoid in order to prevent recurrences.

Stevedores, dockers, lorrymen and the like who have sustained severe disc lesions should be advised to change their occupations for more sedentary ones, otherwise it is quite certain they will have further attacks.

CONCLUSIONS

The vast majority of lumbar disc lesions, with or without sciatica, can be satisfactorily and permanently relieved by treatment on conservative lines. Each case must be treated on its own merits as what suits one does not necessarily suit another. Often a combination of the measures enumerated may be needed to obtain relief. Thus a case may require bed rest, followed by plaster jacket, followed by traction, or vice versa. If severe pain persists in spite of really thorough conservative treatment surgical assistance should be sought, but if sufficient attention to detail is paid this should rarely be necessary.

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[*February* 13, 1952]

DISCUSSION ON THE PLACE OF RADIOTHERAPY IN THE TREATMENT OF THE ARTHRITIC JOINT

Dr. Leslie Hill (Royal United and Royal National Hospitals, Bath):

As a general physician with an interest in rheumatic diseases, I can claim to have had no special experience of this subject. However, at the Royal National Hospital for Rheumatic Diseases in Bath, we are in process of an investigation with the object of assessing the value of deep X-ray treatment in certain of the rheumatic diseases. I should like at this point to express my thanks to Dr. Max Demarais, who is largely in charge of these investigations—under the ægis of the Nuffield Foundation—for his help in the preparation of this paper. It is hoped that when the follow-up period is completed sufficient information will be available to indicate the value of this treatment and the technique and dosage likely to produce the maximum beneficial effect. The investigation is not yet complete, but sufficient cases have been treated and followed up to permit a strong impression of the relative value of radiotherapy in three main groups of chronic joint disease; namely, osteoarthritis, rheumatoid arthritis and ankylosing spondylitis.

Historical aspects.—Since Roentgen's discovery of X-rays in 1895 we have witnessed great advances in the development of X-rays, both as a diagnostic and therapeutic agent. The main impetus of effort has been directed towards the cure of cancer but in the literature we find references to its use in the treatment of joint diseases for many years.

The earliest published report is that of Sokoloff (1897), who reported some benefit in cases suffering from joint diseases. Pfahler (1906), and his associates in America, were the first to use the analgesic effects of X-ray in the treatment of arthritis.

Since then, many conflicting reports on the use of X-rays in the treatment of the arthritides have been published; for instance in England, Watt (1932) reported that in hypertrophic arthritis, radiation was the treatment of choice, whereas in the atrophic type little or no benefit could be expected. Langer (1933) reported good results in the treatment of 363 cases of arthritis by deep X-ray. Garland (1935) recommended the use of X-rays in the treatment of cases of arthritis which had failed to respond to more conventional approaches. His reported results in the treatment of acute gonococcal arthritis were spectacular. Using small doses of X-ray, about 80 r bi-weekly for two to four weeks on 30 patients, he was successful in 93% of cases. Unfortunately no attempt was made to use untreated joints as controls and the numbers are too small for statistical analysis. His results in R.A. and O.A. "while gratifying were neither as spectacular nor as convincing as in the gonorrhœal group". (It is to be noted that the dramatic results obtained in ankylosing spondylitis tended at that time to strengthen the views of those who regarded the ætiology as being of gonococcal origin.) The issue is debatable in a condition in which spontaneous remissions are so frequent.

Smyth, Freyberg, and Peck (1941), at the University of Michigan, made a real effort to investigate, under adequate clinical control, the effect of X-ray therapy on arthritis, in a large series of cases, and found the results promising. Later Smyth, Freyberg and Lampe (1943) reported that X-ray therapy was the best available method for controlling the symptoms of osteoarthritis. In rheumatoid arthritis the results were so discouraging that they decided to abandon the treatment altogether. In a more recent paper Borak and Taylor (1945) reported favourable results in R.A. and attribute their success to the careful selection of cases and the choice of dosage according to the state of the disease. Patients in the early stage of the disease responded the best; but, as the 10 cases they analysed had spinal involvement also, there seems to be some confusion as to whether or not they were dealing with cases of ankylosing spondylitis, which the Americans describe as rheumatoid spondylitis.

In Sweden, Kahlmeter (1938) reviewed experience of radiotherapy over a period of ten years on approximately 5,000 cases of all types of rheumatic diseases. He recommended deep X-ray treatment in all cases of osteoarthritis, advising that the best results are obtained in cases with associated tendonitis, irrespective of the site of the lesion. His results in cases of rheumatoid arthritis were less satisfactory. Osteoarthritis associated with neuralgic pains responded well. Surprisingly, he recommended this treatment in recurrent sciatica but advised great care in the acute stage of the disease.

More recently Weissenbach and Pizon (1948) report favourably on the treatment of osteoarthritis by deep X-ray therapy. They strike a note of warning in discussing the treatment of lumbar region and hip-joints of young female patients, and regard the critical ovarian dose as being in the order of 300 r only.

Gilbert Scott in his monograph on the treatment of ankylosing spondylitis recommends the use of wide-field X-ray irradiation. He first started using this technique in the early 'thirties and reported promising results (1935). The physical factors in his technique are as follows:

130 kV.—3 mm. al filters 60–100 r, half a dose being given anteriorly and half posteriorly at 50 cm. distance. This technique was at first received with great enthusiasm, but it has gradually fallen into disuse. The use of high voltage therapy (H.V.T.) directed to the whole or part of the spine is now the method of choice. No standard method has been accepted and different units use different dosage given over varying intervals. It is now generally accepted that no more than 2,000 r skin dose should be given at any one stage.

Mechanism of relief of symptoms.—No definite mechanism whereby X-ray radiation affects the tissues and relieves pain has yet been fully accepted. The subject is still a controversial one. In 1933 Langer believed that the effect was achieved through the vegetative nervous system. X-rays he argued

induced a better blood supply in the diseased area, promoted tissue metabolism and therefore reduced pain.

Desjardins (1940) suggested that X-rays produce an indirect resolution of inflammatory deposits when these are not yet organized, but Tillis (1944) postulated an effect upon the cellular portion of the blood, believing that the antibodies and metabolites liberated by the destruction of leucocytes in and around the inflamed area are the effective agents; the rate of change following irradiation in the different phases of inflammation corresponding to the rate at which lymphocytes, polymorphs and eosinophils are affected by radiation. The treatment, therefore, he claims should be used as early as possible.

Kaplan (1945) believes that radiation produces its effects by producing analgesia of the involved areas, either through direct action on the nerve endings in the affected area or by relieving inflammatory conditions. This effect is probably due to a direct effect on lymphocytes and leucocytes, reducing inflammation and thus relieving pressure on the nerve sheaths.

Horwitz and Dillman (1944) experimenting on rats found no changes in the bone after large doses of X-ray. They also did joint biopsies on 2 patients with effusion of the knees before and after deep X-ray treatment. They report the following changes in the synovia as being responsible for the control of the effusion which followed deep X-ray. (1) Disappearance of œdema. (2) Increase in fibrous tissue. (3) Thickening of the vessel wall and obliteration of the smaller vascular channels in the synovial layers. (4) Diminution of cellular reactions. Factor (3) might well be borne in mind when prescribing deep X-ray. The vascular changes following large doses may possibly bring about a late avascular bony necrosis.

Two early papers—Ewing (1926a and b)—deal with these tissue reactions and the production of osteitis in a thorough manner.

Ney (1949) reporting an investigation on 9 cases of rheumatoid arthritis treated with wide-field X-ray (L.V.T.) finds that the treatment tends to bring about a return to more normal albumin/globulin levels and that a shift in the protein pattern in the right or the wrong direction foreshadows clinical improvement or deterioration. In believing that L.V.T. stimulates the defence mechanism of the body, he seems to fall in with the views of Gilbert Scott and Hernaman-Johnson (1935) on the constitutional effect of X-rays as determined by blood serum tests. They found that when the vanadic acid test (differential sedimentation test) was positive it was brought back to normal by suitable wide-field therapy. They gave examples of cases of asthma, spondylitis and breast cancer in which the effects of wide-field irradiation could be forecast according to the abnormality of the vanadic acid test.

The interesting observation has been made by Dausset (1930) that, apart from its analgesic effects, deep X-rays check the proliferation of osteophytic outgrowth and thereby relieve symptoms due to their presence (root pressure, &c.). The response in the case of osteoarthritis, where sclerosis, loss of cartilage and cyst formation are the main features, is not so satisfactory.

I suggest that radiotherapy has two main effects, one hyperæmia and the other a temporary suppression of nerve endings.

Techniques.—When the Radiotherapy Centre was developed in Bath it was, as usual, primarily intended for the treatment of malignant disease, but its proximity to a national rheumatic hospital and a large hydrotherapy and physiotherapy centre constituted an immediate and valuable addition to the treatment of the arthritic joint. A preliminary survey of the technique in use in five existing centres revealed an amazing variation; dosage ranged from 100 r to 1,000 r, sometimes given at frequent intervals for a short period and sometimes less frequently over a period of three or four weeks.

During the last three years we have treated over 2,400 cases of various kinds of joint disease (1949 = 700; 1950 = 800; 1951 = 900). By experience we have come to feel that the small dosage given in some centres is useless, while the very high doses given in others are dangerous. We stick to doses in the medium range (from 200 Tissue Dosage in the I.P. joints to 1,200 T.D. in the hips; 1,500 r in the case of the spine) working at 220 kV. with a half-value layer of 1·5 copper. We also found that cases treated slowly (twice a week for three or four weeks) responded better than those treated daily for ten days. In this way skin effect is negligible and other treatment, such as mud packs, pool, &c., can be given at the same time. This is certainly not advisable when heavy dosage is employed or even where medium doses are given frequently. In all cases which have been treated in the Bath Radiotherapy Centre in the last three years with doses not exceeding 1,500 r, there has been only one case showing a skin reaction. In this medium range, therefore, there is no fear of skin reaction or bone necrosis and any unfavourable reaction in the joint is likely to be slight.

DISCUSSION OF RESULTS

The final answer to the value of D.X.R. in the treatment of rheumatic conditions will be available only when a large series has been critically analysed under controlled conditions. In order to ascertain whether improvement in the joint treated is due to deep X-ray, it is necessary to correlate such improvement with any change in the disease as a whole, as indicated by the general condition of the patient and the state of the other affected, but not treated, joints.

Pain being entirely subjective, it is necessary to rely on the patient's statements. In order to simplify the interrogation as much as possible, it has been graded into pain at rest, pain on movement and/or on weight-bearing.

The course of rheumatoid arthritis is unpredictable and general and local remissions and exacerbations are characteristic. The results of any form of treatment can often be misinterpreted and a false deduction made. It is perhaps fairly generally agreed that deep X-ray therapy has no place in the local treatment of an acutely inflamed rheumatoid joint. It can be better employed for those cases which fail to respond to other forms of treatment and in which one or two joints are holding up the efficient rehabilitation of an otherwise quiescent case. Even with careful selection, it is not uncommon to see a rheumatoid joint flare up under the influence of deep X-ray, and, in the present state of our knowledge, the reason for this is not easy to see. I am not entirely satisfied that radiotherapy is of use in rheumatoid arthritis except as a very temporary measure. It is certainly true that in many cases it makes things worse. Dr. A. A. G. Flemming has stated, in conversation with me, that in treating the rheumatoid joint, he would never, from choice, exceed the small dosage (100 T.D. for I.P. joints—500 T.D. for hip-joints), and, even then, he would review the case after the first dose to note any local or general reaction.

The response of the osteoarthritic joint to deep X-ray therapy is more favourable, and alleviation of pain in this condition is sometimes dramatic if the dosage has been carefully selected for the type of case. However, some fail to show the expected response. The reason for this is not, at present, apparent, but Dausset's observation that cases in which osteophytic lipping is a feature respond better than those with predominantly degenerative changes, is worthy of confirmation. In cases of osteoarthritis of the cervical and upper dorsal spine, with root symptoms, the referred pain is very often relieved to a greater extent and more frequently than the local spinal symptoms. In the lower spine, pain and referred sciatic symptoms are more refractory to treatment. Night and rest pain in the hips and knees resulting from osteoarthritis is often relieved by deep X-ray and the treatment is well worth giving when such pain interferes with sleep and the well-being of the patient. Where there is gross destruction of the joint, in my opinion, very little benefit accrues, and what there is temporary. In osteoarthritis we, in Bath, are convinced that radiotherapy is more efficacious when combined with other forms of physical treatment, particularly the deep pool. When the physician in charge of the case, or the orthopædic surgeon, has decided what other form of treatment is desirable, the radiotherapist can then estimate the dosage and frequency of treatment.

In ankylosing spondylitis the wide-field technique of Gilbert Scott is no longer, in our opinion, the treatment of choice. High voltage therapy directed to the whole or part of the spine and the sacroiliac joints is now preferred. Various centres use different dosages given at varying intervals, but it is now generally accepted that no more than 2,000 r skin should be given at any one stage and the results are very satisfactory. In Bath, we are not entirely in agreement with those schools who consider it necessary to go up to 2,000 s/r. Our results seem to be quite as good with 1,500 s/r. We are also satisfied that it is better to treat the whole spine rather than portions of it. Not only do we find the results better, but it avoids the difficulty met with when, having treated the upper thoracic spine six months before, a request for treatment of the lumbar spine comes in. In such circumstances it is a tricky business to avoid either leaving a gap or overlapping and running the risk of local overdosage.

Whether the treatment arrests the progress of the disease is a debatable question. Oppenheimer (1943) claims complete cure when treatment is initiated in the very early stages of the disease. The "forme fruste" of ankylosing spondylitis (bilateral sacro-iliitis which does not progress to the full picture of ankylosing spondylitis) is not an uncommon variant of the disease and the arrest of progress in the sacro-iliac joints could easily be attributed to treatment if adequate controls are not followed up at the same time. We are all agreed that, apart from a few very acute cases, the evolution of the disease tends to be a slow process and many cases end in complete ankylosis of the spine without any symptoms throughout the history of the disease. It is generally observed that following deep X-ray therapy in ankylosing spondylitis the pain in the spine is almost invariably relieved and, if the disease is not far advanced, increased mobility accompanies relief of pain. The general condition of the patient improves. The effect on the sedimentation rate is inconsistent but a falling rate often runs parallel to the clinical improvement.

RESULTS AND CONCLUSIONS

(*a*) *Rheumatoid arthritis.*—The figures to date suggest that radiotherapy in this condition is of doubtful value. It is certainly contra-indicated in the acute joint and should not be used in debilitated individuals. It may have a use in individual joints which hold up progress in an otherwise quiescent case.

(*b*) *Osteoarthritis.*—Results are variable, but, on the whole, promising. Relief of pain due to spinal osteophytic lipping can be more often expected in the cervical and upper dorsal region than in the lumbosacral. It tends to be more effective peripherally than centrally. The results, where sclerosis, loss of cartilage and cyst formation, are the order, are much less certain, and on the whole disappointing.

Medium and high dosage are equally effective, but medium dosage is to be preferred, since it can be, if necessary, repeated, and can be given in association with other valuable methods of treatment, especially mud and exercises in the deep pool. The high dosage may produce better results than the medium when pain on movement and/or weight-bearing is the prominent symptom.

(c) *Ankylosing spondylitis.*—The treatment is undoubtedly of great value, both in the relief of pain, the restoration of movement and also, it seems, on occasions, in the arrest of the disease process. The effect of radiotherapy on the course of the disease and on the length of the remissions will only be apparent after a long-term follow-up. The wide-field, low-dosage treatment previously largely employed, has now given place to medium or high voltage therapy directed to the spine or sacro-iliac joints. Medium and high dosage seem equally effective.

Deep X-ray is only a local treatment and the care of the patient on general medical lines must never be neglected. Physiotherapy, hydrotherapy, orthopædic measures, intravenous iron, blood trans-fusions remain the main planks in the treatment plan. The chief danger of deep X-ray therapy, particu-larly in rheumatoid disease, is the effect on the hæmopoietic system. It is advisable to keep a close watch on the white cell count, especially when large doses are used, and care should be exercised when the total white count falls to within the 3,000 limit. Mild pigmentation of the irradiated areas is not rare but severe damage to the skin in expert and experienced hands should never occur. Cases some-times relapse after the first course of treatment, and for this reason the minimum effective dose is the one of choice, since it can be repeated at a later date, if and when necessary.

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Professor B. W. Windeyer (Middlesex Hospital, London) discussed his experience in the treatment of four conditions: (1) Sub-acromial Bursitis; (2) Rheumatoid Arthritis; (3) Osteoarthritis; (4) Ankylosing Spondylitis.

(1) *Sub-acromial bursitis.*—25–30 cases had been treated. In the majority the condition had been present for several months and a variety of forms of physical medicine had been used with no lasting improvement. X-ray treatment had been used with a beam generated at 200 kV. and a half-value layer of 1·4 mm. Cu. Two opposing fields were used to irradiate the affected shoulder and a dose of between 1,200 and 1,500 r was delivered to the centre of the joint in three weeks. The maximum clinical reaction produced was a mild erythema of the skin.

By the end of the course of treatment there was usually some improvement in that the constant aching pain had become less and there was slight decrease in the marked limitation of movement of the joint. Within three to four weeks of the completion of treatment the pain had disappeared and a full range of painless movement was soon regained.

The cases with a shorter history appeared to benefit more rapidly than those whose symptoms were of longer duration.

(2) In *rheumatoid arthritis* his experience was that X-ray therapy had not caused improvement in limiting the course of the disease or relieving pain in patients with acute disease. X-ray therapy

appeared to be of benefit in reducing the amount of periarticular thickening in some patients when acute symptoms had subsided. He had used the same quality of radiation, i.e. 1·4 mm. Cu half-value layer. and thought that there had been no appreciable difference in the response when weekly doses of 200 r had been repeated three or four times as compared with a more intensive dose of 1,200 r over three weeks.

(3) *Osteoarthritis.*—X-ray therapy was a useful palliative for the relief of pain in osteoarthritis. It appeared that in many patients a greater degree of relief could be obtained by X-ray therapy than by methods of physical medicine. Palliation was obtained in about one-half of the patients treated. The reduction in the amount of pain, in some patients almost complete abolition of pain, usually lasted only for a few months, but in some there had been real improvement for periods of two years and more.

In the remaining 50% of patients there did not seem to be any response at all. He was unable to say which patients were most likely to respond. It did not seem that it was a question of the early case or the late case. There had been both good response and failure in cases which were in the earlier stages, and also in those where there was marked cartilage loss and bony deformity. In the last 60 patients treated 27 had definite improvement in the amount of pain and 33 had no improvement.

The technique used had been to give a dose of 1,200–1,400 r to the affected joint over three weeks, with a beam of 1·4 mm. Cu half-value layer.

Patients who had received benefit were usually very anxious to have the treatment repeated when the pain returned. It was not possible, however, to repeat such a treatment more than once or twice without running a risk of producing radiation changes in the skin and subcutaneous tissues. It seemed to be advisable to insist that X-ray therapy for the palliation of osteoarthritis should be used only when methods of physical medicine, which were simpler and potentially less dangerous, had been tried and found to be ineffective.

(4) *Ankylosing spondylitis.*—X-ray therapy was of the greatest value in the treatment of ankylosing spondylitis and had revolutionized the treatment of this condition. After a course of X-ray therapy, in almost all cases with active disease, there was relief of pain and relief of muscle spasm, so that the patient was able to move with greater freedom and rest in comfort. In early cases when calcification of intervertebral ligaments or destruction of apophyseal joints had not occurred, posture could be restored to normal and a full range of movements could be regained. Even in later cases some improvement could be obtained in both posture and movement by reduction of muscle spasm and then re-education of wasted muscles.

In those patients in whom the disease ran a fairly acute course with pain, anorexia, fever, night sweats and loss of weight, there was usually a marked improvement in general condition. Their state of anxiety was relieved; they became afebrile and able to sleep and gained in weight.

In advanced cases, where there may be involvement, in addition to the spine and sacro-iliac joints, of a multiplicity of joints and muscle attachments, such as hips, symphysis pubis, sternoclavicular joint or ischial tuberosities, the disease may not become completely quiescent and in these cases recurrence of symptoms may occur, usually but not invariably in some area which has not already been treated.

In the majority of the earlier cases the disease became completely quiescent and there had now been a considerable number of patients under observation for periods of over ten years who had had no recurrence. It was a disease which might last for many years and which may be subject to natural remissions and relapses in the absence of treatment. It was therefore not possible, without a large number of patients with prolonged periods of observation, to state whether ankylosing spondylitis could be permanently controlled by X-ray therapy. At the Middlesex Hospital over 800 cases had been treated since 1939. An analysis of 277 of them with the results of treatment was made by Miss M. D. Snelling in 1949.

The early diagnosis of this condition had become increasingly important. The earlier the case came for treatment the more complete was the restoration of normal function and the more permanent the result. There was now a much greater emphasis on early diagnosis and many patients were referred in the stage when the only radiological sign was early alteration in the sacro-iliac joints. Care was particularly necessary in such early cases because there had been a considerable number referred with a view to X-ray treatment because of low back pain and radiological evidence of irregularity and areas of sclerosis, and decalcification of the sacro-iliac joint margins, who certainly did not have ankylosing spondylitis. There had been cases of tuberculosis, of post-traumatic arthritis, and of prolapsed disc. Cases with unilateral sacro-iliac involvement were particularly doubtful. Ankylosing spondylitis may be unilateral in the sacro-iliac joint and may remain so for a considerable time. Cases had occurred in this series where unilateral disease was found, and the diagnosis was doubted and the patient kept under observation without treatment. At later radiological examination, perhaps after an interval of a year, it had been found that the disease had spread to involve the other side.

Treatment.—The technique of treatment had been fairly uniform. The X-ray treatment which had been used consisted of a single course of treatment in which the whole of the spine and sacro-iliac joints were irradiated. Five fields were used; the quality of X-ray beam was 1·5 mm. Cu half-value layer. 2,000 r were given incident to each of the five skin-fields in a period of four weeks. The whole of the spine and sacro-iliac joints were treated as a routine in each case. Other joints and muscle attachments were treated only if there were symptoms or signs that they were involved. An integral and essential part of the treatment was a course of exercises designed to promote movement and to restore the function of muscles which had wasted from disuse. In particular, deep breathing exercises were given. If the patient were in an acute phase of the disease it might be necessary to give these exercises very gently for a prolonged period of time, and also to use slings and pulleys to help the wasted muscles. As the patient grew stronger he was put into a class with other patients in the Department of Physical Medicine. Orthopædic appliances such as plaster beds were retained to allow the patient to rest and to prevent further deformity until the muscles were sufficiently restored to overcome this danger.

Complications.—During a course of treatment there may be and very frequently was a marked drop in the white cell count. This had frequently reached such a stage that it had been necessary to interrupt the treatment for some days, or even a week or two in extreme instances. The proportion of women in the series was only 8%, but they were almost always young women and there was danger of affecting ovarian function if the sacro-iliac joints were irradiated. A technique had been tried which treats the sacro-iliac joints obliquely from behind and forwards, using the edge of the beam, so as to miss the ovaries. This appeared to have been successful and since its introduction there had not been cases of amenorrhœa. Some patients had had a successful pregnancy following this treatment.

There had been one patient who developed chronic myeloid leukæmia some years after full irradiation of the spine and sacro-iliac joints, and it was noted as a possible late complication of this quite extensive irradiation. This was the only patient in the series of over 800. It was probably only a chance occurrence of these two conditions. Another patient who had been referred for radiotherapy on account of chronic lymphatic leukæmia had been found on admission to have quite advanced anky-losing spondylitis which had never been diagnosed and never been treated.

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SAMUEL HYDE MEMORIAL LECTURE

NUMBER 10

The Evolution of Physical Medicine

By FRANK D. HOWITT, C.V.O., M.D.

I DEEPLY appreciate the honour you have conferred on me by asking me to deliver the Samuel Hyde Lecture, and particularly on the subject you have chosen for this occasion—"The evolution of Physical Medicine". Your timing and your choice of place for such a discussion are, moreover, particularly fitting, because it was in this very house, exactly twenty-one years ago, that Physical Medicine, as an acknowledged speciality, was born. That would appear to suggest that the subject, having passed through its teething troubles and its adolescence, has now come of age, and stands on an equal footing with the other senior constituent members of the Society. I shall confine myself to the evolution of Physical Medicine in this country.

The parents of Physical Medicine in the Royal Society of Medicine were the old Sections of Balneology and Electrotherapeutics. The Section of Electrotherapeutics consisted in part of radiologists, whose subject by this time had so far advanced that they had material and to spare to form a Section of their own, and in part, of doctors interested in conditions which responded to treatment by physical methods—movements, manipulations, exercises, electrical and ray treatments and so on. Their discussions included such subjects as "The work of a Light Department", "Dia-thermy in Relation to Circulatory Disturbance" and clinical meetings on "Diseases of Joints". In reviewing the subject matter of these meetings, it is evident that the predominating theme was the application of the basic sciences to medicine. Indeed, the President of the Section for the year 1921 chose as the title of his Address "The importance of Physics, Physiology and Anatomy".

The Balneological Section consisted largely of Spa doctors, specializing for the most part in rheumatism, and other medical disorders of the locomotor system. Their discussions were also of

a clinical nature, but with special emphasis upon climatology and applied physiology, and included such subjects as "Diathesis in Relation to Rheumatism", "Geographical and Climatic Distribution of Rheumatism" and "The Spa Doctors' Contribution to the Improvement of National Health".

It seemed obvious, therefore, to the Committees of our parent bodies, whose membership was almost identical, and whose work had been conducted on parallel lines, to pool their interests, and inaugurate a new Section. And so, on March 20, 1931, a Resolution was passed "that this Section of Balneology and Climatology fuse with the Section of Electrotherapeutics to form a new Section of 'Physical Medicine' ".

The year 1931 can then be taken as the date in which Physical Medicine became established as a fully recognized member of the hierarchy of Medicine, but it was by no means the first occasion on which the term had been used. It is necessary to go back into the history of the major hospitals, many of which had appreciated, for several years previously, the need for a department which could deal with those cases in which physical treatment was indicated, and those which, having passed the stage of hospitalization, were in need of re-education. These departments were at first placed under the titular supervision of some member of the hospital staff, often a radiologist, who had no primary interest in the subject. The scope of their work was almost entirely therapeutic, and the treatments commonly empirical. In the course of time, physicians were appointed for the sole purpose of taking clinical charge of these departments, and they passed under a variety of different names. The first hospital which designated its physician-in-charge under the title of Physical Medicine was the London Hospital, and this pioneer appointment was made in 1921.

At first sight the title does not appear to be perfect but it has defied all efforts to find a better one. In a somewhat ill-defined way it seemed to suggest the study of physique, its abnormalities, its deviations, and of its rectification by physical means. This outlook had played a major role in the studies of both of its progenitors, and thus seemed to effect a happy compromise. The Greek word φύσις means "nature", and it is the word from which are derived the titles of the arts and sciences of Physic, Physics and Physique, and of the adjective "physical"—which owes common allegiance to them all. On reflection, therefore, it will be seen that the title Physical Medicine is, in reality, a particularly apt one, and it also provides for a healthy measure of elasticity.

But of greater importance than the name was the sphere of activity which the new Section was to adopt. The Committee reviewed its inheritance. It realized that there was in its possession a wide field not yet encompassed by any other branch of medicine. It realized also that the men who came together under the banner of Physical Medicine at that time, no less than today, represented seemingly different facets of medical endeavour. Some had specialized in somewhat restricted clinical fields, others in particular forms of treatment, based for the most part on applied anatomy, physiology and physics. Yet they felt that in Physical Medicine they had found an outlet, an expression and a platform for their work. And it was the primary duty of the new Section to integrate their endeavours and to find a common denominator which would be acceptable to all; to synthesize their separate approaches; and to weld them into one concrete whole. That common factor was not apparent at the time, but looking back upon the evolution of Physical Medicine, I say, without fear of appearing sententious, that the bond which has held the subject together and strengthened it is a medical philosophy.

The main spheres in which Physical Medicine has worked come under three headings:

Firstly, the management of certain diseases of the locomotor system—in particular the rheumatic diseases—and of the chronic sick; secondly, the restoration to maximum efficiency, following disease and disability; and thirdly, the achievement and maintenance of health.

Many subjects were discussed here in the early days of the new Section, but I shall mention only a few—"The Effect of Strain on Children", "Physical Education", a debate on the Need for the Promotion of National Physical Fitness, which took place eighteen months before the last War, and which elicited much abuse as to its apparently militaristic flavour, "Physiological Aspects of Rheumatism", and so on. What do these connote if not a social contribution, a wide and purposeful contribution to the welfare of the State? It was not until later, under the duress of national emergency, that the profession, as a whole, awoke, not only to their importance, but to their necessity, and new names were coined for them, including that clumsy omnibus term "Rehabilitation".

Slowly the contribution of Physical Medicine became recognized, and one by one the great hospitals appointed physicians in this subject to their staff on equal terms with those granted to other specialities. Finally, in a few instances, beds were allotted.

As might be expected, there were obstacles to be overcome, and these can be included under two headings. In the first place, watchful eyes and even sidelong glances were cast upon it, by individual members of other specialities, on whose provinces, at first sight, it appeared to encroach. Amongst these I may mention orthopædic surgery, itself a young, but already a most important and virile member of the medical community. This watchfulness was not only natural, but proper and correct,

and it quickly disappeared when it became evident that no such intention existed. And, in the result, we number amongst our most intimate colleagues the members of those specialities whose objectives are commonly the same as our own, and with whom we have formed such a happy liaison.

The other main obstacle to progress, and one which has been found more difficult to dispose of, was the illusion that Physical Medicine was purely a therapeutic speciality. This also was natural enough in those early days, for time was needed, particularly amongst the older members of the profession, to appreciate the emancipation which had taken place in hospital departments, since the old days of passive and palliative treatment, and consequent upon the new appointments of clinicians to take charge of them.

And so the Speciality of Physical Medicine advanced, tardily perhaps, but surely, until the outbreak of war in 1939. Someone has defined Peace as the interval between wars. In peacetime, medicine relaxes, takes stock of the situation, consolidates, and devotes itself to research and academic advancement. In war, there is a need of urgency, of adaptability, of improvisation, and above all, of purposefulness. The war afforded to Physical Medicine a great opportunity in certain aspects of its work.

There was in this country an urgent need to make full use of manpower. The substandard had to be trained to a higher grade. There was a pressing need for speedy reinstatement of the sick and disabled to maximum fighting capacity, or if that was not possible, to a new vocation, which would utilize to the full their resultant powers. The selection of the right man for the right job became a national necessity, as did also his specific training for that job. In all these duties the main burden fell upon Physical Medicine. The task was eagerly accepted and specialists were allotted both to the fighting and the civil services.

In the Army, in association with the Directorate of Army Health and with the Army Physical Training Corps, the duties of Physical Medicine fell under three headings: Advice on Training, Physical Development, and Rehabilitation. The preventive role of Physical Medicine in the Army was defined in 1942 as follows:

"Full use should be made, by Unit Commanders and especially Commanders of Training Units, of advice given by Command Specialists in Physical Medicine as to how training can best be organized. Such advice should be used to improve the fitness of men recently enlisted, and to alleviate the risk of impairing their physique by training which is intended to improve it."

Physical Medicine specialists accordingly visited Primary Training Centres, Corps Training Centres and Field Forces Units, with the object of advising upon progression in training (a most important factor), the conservation of energy, and the purposeful training for the specific requirements of the different arms of the Service. Research was carried out into such problems as the determination of a man's best age, the age for attaining maximum efficiency with regard to strength, agility and endurance, and the relative value of clinical data such as vital capacity estimations, exercise tolerance tests and various anatomical measurements; of the effect of fatigue in relation to performance; and on many problems of this kind. Physical Medicine also assisted in the problem of personnel selection for Army employment, and in the evolution of the Pulheems System which came to replace the old methods of Army categorization.

Physical Development Centre

Because of the intense demand for manpower it became of paramount importance to attempt to up-grade to fighting capacity the substandard of the Country—that is to say, those recruits which had been relegated to low categories by examining civil medical boards. With this object in view, the Army, which had always paid particular attention to improving the physique of substandard recruits, instituted in 1941 a new form of Depot known as a Physical Development Centre. This centre, and others which followed, were initiated, organized and controlled under the joint direction of Physical Medicine Specialists and of officers of the Army Physical Training Corps. The selection of cases to be referred to these centres was made by Specialists in Physical Medicine. The disabilities found most likely to respond included poor physique due to under-development and malnutrition, abnormalities due to postural, occupational and environmental causes, and localized skeletal defects not associated with rigid deformity.

The regime was built up on teamwork in which the physical, psychological, educational, welfare, and nutritional viewpoints were all borne in mind. A corrective and remedial system of group exercises was evolved to counteract the defects of locomotion and physique, most commonly encountered. Individual treatments were undertaken, when necessary, by physiotherapists of the Chartered Society. Chiropodists had a most important function to fulfil. Every adolescent received a thorough dental overhaul. The value of diet, both quantitative and qualitative, was studied, and particular attention was paid to the cooking, and the manner in which meals were served, as the psychological effect of this is considerable. At intervals during the course, the recruits were medically examined, and the final assessment and categorization were made by Physical Medicine specialists, based on performance tests; and advice given as to their future placement in the Army in each case.

The success of the first centre was such that three further centres were opened, and in the result, many thousands of substandard recruits were elevated to the highest category, and remained in this category until the end of their Army career. The third sphere of the activities of Physical Medicine in the Army was the final stage of rehabilitation of the damaged soldier, which took place at the Military Convalescent Depots. The title was a misnomer because the depots served, in fact, the function of re-training and hardening him for further military service. The condition in which the patients arrived varied within wide limits, depending upon the nature of the disease or disability for which they had been treated, the previous length and type of hospitalization, and the individual mental and physical constitution. The nature and the degree of re-training necessary were also dependent upon the physical demands of the different arms of the Service to which the soldier would be returned, and on the final category which he was expected to attain. For these purposes it was important for the Physical Medicine specialist to acquaint himself with these different demands, and so to advise upon re-employment in those cases in which a change of vocation was necessary.

In the R.A.F. particular attention was paid to the special problems relating to the achievement and maintenance of physical fitness in flying personnel.

Rehabilitation in Hospital

The main burden with regard to the hospital aspect of rehabilitation fell, however, upon the Physical Medicine Specialists of the Emergency Medical Service, working in conditions of great difficulty, in a vast network of civilian hospitals and other institutions specially erected, or converted for this purpose. Perhaps the most notable advance accomplished by this group was the remarkable spirit of team-work and co-operation which was achieved. Physical Medicine, which, in its early struggles before the war, had been relegated to somewhat isolated departments, now worked hand-in-hand with general surgeons, orthopædic surgeons, general physicians, neurologists and so on, and became the cement in the vast fabric designed to recondition to maximum capacity and in the minimum of time, the casualties of the Services and of the civilian population.

Then came Peace. The problems of Physical Medicine were reversed. The pre-war struggle for recognition in the medical hierarchy had disappeared. Not only the subject itself, but its philosophy had been accepted. The difficulty now was to maintain and consolidate its position, and the gains which had been made both before and during the war.

British Association of Physical Medicine

In all these problems, the British Association of Physical Medicine, which had been formed in 1943, exerted a controlling influence. Its Policy and Education Committees took over the general direction of the education and training of intending recruits to this branch of medicine, the field which the subject could effectually encompass, and the standard of qualification which would be adequate for full consultant status. There were not sufficient fully trained men of high medical calibre to fill the posts now open to them. Faced with the difficulty of training new entrants to meet this situation as quickly as possible, certain makeshift expedients were devised. But the Council now requires the highest medical qualifications, in addition to a long apprenticeship in a Department of Physical Medicine, as pre-requisite for full consultant status.

The Scientific and Technical Committee of the Association has drawn freely upon the admirable work emanating from St. Thomas's Hospital, not only in the pioneer field of electro-myography, but also in the evaluation of different kinds of apparatus and techniques, and the advice to be given to the main technical auxiliary of Physical Medicine—the Chartered Society of Physiotherapy. It has reached a standard of ethical and technical excellence unsurpassed in any country in the world. The syllabus is now of 3 years' duration, and this training is under the medical direction of the Physicians-in-Charge of Physical Medicine Departments, who also assist with the final examinations.

Post-War Civilian Centres

There was a widespread demand for the establishment of Civil Centres based on the wartime lines. The late Mr. Ernest Bevin, when Minister of Labour, was greatly impressed by the Services Centres of Rehabilitation and Re-vocation, and insisted on the creation of similar centres for the residential reinstatement and re-education of civilians. The first of these centres was opened at Egham. It admits both post-hospital patients, for the later stages of their resettlement, and cases of industrial misfits, the latter because of some specific disability, who are unable to continue in their previous employment. All forms of physiotherapy under medical supervision and also a wide range of work-shops and sedentary occupations are available, each under a qualified teacher. The patient is inter-viewed on admission, and at intervals during his stay at the Centre, by the doctor, in consultation with an officer selected for his knowledge of the requirements of the various branches of industry. His capabilities are thus assessed, his re-training supervised, and his re-vocation arranged accordingly.

Many large firms have established rehabilitation centres of their own, reinstating their employees within their own fabric, but in the case of the small firm the problem is more difficult because re-instatement can only be satisfactorily undertaken in organizations in which a wide diversity of

occupation is available. To solve this problem the Nuffield Health and Hospital Service has launched a comprehensive pioneer scheme at Slough. The objects of this Centre are, firstly to offer advice in relation to selection of vocation, and to the prevention of occupational disease or injury; secondly to provide facilities for rehabilitation and re-employment, including a recuperative home; and thirdly, to carry out research into the different problems of industrial rehabilitation.

The Ministry of Health is taking great interest in this subject, as evidenced by the institution of the recent residential centre of Garston Manor. Here an integration has been effected with the Industrial Rehabilitation Unit of the Ministry of Labour, and certain cases are referred to this Unit for assessment and training, whilst undergoing medical treatment at Garston.

And finally, mention must be made of the Disabled Persons Employment Corporation, whose function is to provide employment in its factories, workshops, and in the case of the homebound, in their homes, for all classes of registered disabled persons who are so severely handicapped as to be unlikely otherwise to obtain employment. Employment is provided in factories known as Remploy factories, of a type suitable for severely disabled persons.

All these major enterprises to which I have referred are conducted under the medical guidance of Physical Medicine Specialists, or of doctors who learned their work in Physical Medicine Departments.

With the advent of Peace also, the centre of gravity returned to the hospitals. In almost every teaching hospital in England today, there is a Consultant Physician in Physical Medicine. In his department two groups of patients will be found. In the first place he takes charge of out-patient clinics, on the same basis as other consultants, and in many instances beds have been allotted to him although in the latter instance, because of the shortage of in-patient accommodation, this has not yet been fully achieved. To his out-patient clinics, general practitioners refer those cases for which his department is specifically designed and equipped to deal, that is to say, cases of chronic rheumatism, and arthritis, certain œdemas and peripheral vascular disorders, and so on. These cases, together with those transferred to him by his hospital colleagues, as coming within his special province, remain his primary responsibility. In the second place, the Physical Medicine Specialist receives patients referred to him by his hospital colleagues for advice on the value and the application of physical treatments in a wide field of medical and surgical conditions. These patients, although treated in the Department of Physical Medicine, under the supervision of the Physician-in-Charge, are, as it were, his guests, and remain the primary responsibility of the referring physician or surgeon to whose care they are returned on the termination of treatment.

With regard to the type of case which has become recognized as the primary province of Physical Medicine, the Specialist is faced with problems of a wide and varied nature. Many of the patients so referred complain of some specific disability, such as pain, stiffness or weakness, for which the application of some form of physical treatment, either alone, or in conjunction with other appropriate medication, would appear to offer the most hopeful means of relief. But before any form of treatment can be prescribed, it is, of course, essential that a full and thorough clinical examination be undertaken together with such investigations—pathological, biochemical and radiological—as may be indicated in each individual case. Every Specialist in Physical Medicine has had ample experience of cases, so referred, whose symptoms are found on clinical examination to be due to some primary neurological disturbance, such as disseminated sclerosis; to secondary deposits in the spine or elsewhere; to pain referred from the chest or abdomen; and to a multitude of different causes. These cases are, of course, transferred immediately to the appropriate department concerned.

Moreover, many of the patients of Physical Medicine require a close assessment of the psychological factor. One does not have to study the writings of Plato and other ancient scholars, nor to call upon the more modern concepts of psychosomatic medicine, to appreciate the fact that physical disability, if long continued, will produce a degree of mental trauma, which will manifest itself in many different ways. On the other hand, psychogenic factors are capable of producing physical disabilities, and certainly of aggravating them.

The assessment of pain is a difficult matter, and its effect on the endurance of a patient varies with the individual, for each has a different threshold. Subjective symptoms, therefore, bear little relationship to their cause. What one patient describes as "agony", will be dismissed by another as of small account. A disability is a challenge to the strong, an excuse to the weak. When malingering is suspected, it must be remembered that there are two kinds of malingering. There is the man who realizes that the mere fact that he is under treatment is sufficient justification for a certificate for absence from work. On the other hand, there is the man—a craftsman perhaps, an artist or a sportsman, who is passing his zenith, and the symptoms of which he complains are in reality an excuse mechanism for failure any longer to excel. Then there is the man in whom not only increasing disability, but the fear of it, and consequent loss of employment, is a vital factor. In all these cases, a sympathetic evaluation must be made, and treatment ordered or refused accordingly. Lastly, there are the patients complaining of symptoms for which, on clinical investigation, no pathological

abnormality can be discovered, but in whom, on detailed enquiry, some extraneous cause will be found. Take, for example, the case of a lorry driver presenting himself with pain in the low back, sciatica, fibrositis of the neck or brachial neuralgia. In addition to examining the man, it is important to enquire into the lorry. It may be found that the driving space is limited, that the gear has been placed in an awkward position, or that the clutch is extremely stiff. The back support, the function of which should be, not only to maintain a normal sitting position, but also to act as a fulcrum for the necessary leverage, may be found inadequate in size, angle and adjustability. Take again the case of the young typist or sedentary worker, complaining of fatigue, both mental and physical, lassitude and lack of concentration. Whilst treating the anæmia or other medical conditions found, it will be as well to enquire into the environmental factors. It may be found that the workroom is maintained at an unnecessarily high temperature; it may be noisy owing to the nature of the work in progress. The lighting may be inefficient, and the air ill-conditioned. A most important point, often not sufficiently considered, is the effect of boredom due to the endless monotony of the work. These conditions must be rectified, and general advice given to such a patient, particularly with regard to relaxation, and the proper use of leisure. It is the tense individual working in an exacting occupation who seldom realizes the need for, or acquires the art of complete relaxation. It is the man who is doing a dull and boring job who will benefit by some degree of excitement when off duty.

The effect of stress, both mental and physical, has for long been recognized as a potent factor in the causation of many diseases, although it is only recently that laboratory proof of this has been forthcoming. Such cases are commonly encountered in a department of Physical Medicine. For instance, the detection of the prodromal symptoms of rheumatoid arthritis before the onset of joint manifestations is of first importance. On careful enquiry into the early history, it will be found that in childhood these patients were unduly frail, and prone to all sorts of juvenile ailments. In adolescence they were nervous and excitable, and alternated between periods of enthusiasm and depression. Undue fatigue was a noticeable characteristic. Although mentally alert and physically active, they soon became exhausted, listless and irritable. Their vasomotor system was unstable, so that they were susceptible to cold. They were thin, somewhat poorly developed, and subject to cramp and vague muscular pains. If such a patient is examined shortly before the first onset of the familiar joint swellings, the following syndrome will commonly be found: loss of appetite, tachycardia, a slightly raised temperature, tremor, general nervousness, loss of weight, and an extreme degree of fatigue disability. A careful watch must be kept on all such patients.

I mention these few instances as typical of the type of case encountered in the everyday practice of a Physical Medicine Department, because they may serve to demonstrate the degree of insight and experience that is required to deal with them, and also, perhaps, the welfare aspect of the speciality which is implicit in its philosophy. Every Physical Medicine Department has or should have the service of a full-time almoner or almoners, and full use is made of their help and enquiry, particularly in regard to the preventive aspect of certain cases. The early detection of environmental factors, domestic difficulties, incompatibilities and frustrations; housing and climatic conditions; occupational stresses and exposures, is essential, as these are potent causes of incipient disease, and their rectification can often prevent irretrievable disaster. In the field of rehabilitation and resettlement also, the assistance of the Disablement Rehabilitation Officer is obtained in relation to the opportunities available for the re-vocation and re-training of the seriously disabled patient.

The Modern Concept of Physical Medicine

In the evolution of things, it has come to pass that Physical Medicine embraces three main aspects of Medical Science. Firstly, there is its hospital duty of taking charge, both from the preventive and remedial aspects, of a wide and somewhat poorly defined group of conditions, particularly of certain disorders of the locomotor system. Secondly, it takes charge, in the later and post-hospital stages, of the re-education, re-settlement, and re-vocation of disabled persons, except in certain special instances, such as that of the totally blind, where segregation is necessary. Thirdly, it advises on problems concerning positive health, and in particular those concerning physical education and the upgrading of substandard youth. At first, it might appear that these three spheres are separate entities, and the criticism has been made that the Speciality, as it has evolved, is too wide and too diffuse. Yet on further examination it will be seen that this is not the case, because Physical Medicine is primarily concerned with the management of the chronic sick, and with their reinstatement to maximum capacity. In the achievement of this purpose, although the conduct of every case may also involve the use of drugs, diets and other medication, the value of physical methods predominates.

It seems to me, therefore, a pity that certain specialists in Physical Medicine should have shown a desire to fragment the subject, by the creation of a separate "speciality" of Rheumatology. I do not wish to infer that those organizations which are doing such excellent work in drawing the attention of the public to this crippling group of diseases, and those which are undertaking advanced research into their ætiology, are not of the greatest value, and Physical Medicine will continue to give them every support in its power. But from the point of view of practical application, it is not possible, to my mind, to specialize in Physical Medicine for any considerable length of time without acquiring

an expert knowledge of the rheumatic diseases. Neither is it possible to advise with any hope of success on rheumatic conditions, without a considerable knowledge of Physical Medicine. Specialists in Physical Medicine, including those who have made rheumatism their special interest, would not wish to delimit their field in this way. It is true that the brilliant discovery of cortisone has provided a most exciting advance in knowledge, but practising clinicians should not allow their attention to be unduly deflected thereby. The enthusiastic reception which greeted the initial spectacular improvement effected in cases of Rheumatoid Arthritis has given way to a more guarded and critical appraisal. It would now seem that whereas the symptoms of the disease are held in check, the disease process remains uncontrolled. There is obvious danger, therefore, quite apart from the possibility of producing serious side-effects, in abolishing the reaction of the tissues to inflammation, and in masking the warning signals of disease. And even if, in the light of eventual knowledge, these or future discoveries, based perhaps on some entirely different hypothesis, should succeed in controlling Rheumatoid Arthritis before the onset of permanent joint changes and incapacity and keeping it, unassisted, safely in check—this condition will still remain but a small fraction, even of the rheumatic group. A far greater number of rheumatic patients suffer, for instance, from the misfortune of growing old, and until a drug is discovered which succeeds in putting back the ravages of time, there will still be much to be done, even in this field.

The Future of Physical Medicine

This is part of general medicine. The last lingering belief amongst the profession that it is purely a treatment speciality must be dispelled for ever. No more must we hear of patients being referred to a medical consultant for physiotherapy. I remember the time, not so very long ago, when the same confusion arose over the terms radiology and radiography, but that is a thing of the past, owing to the stature which Radiology has deservedly achieved.

Academic and Whole-time Appointments

It seems to me, also, a pity that so many wholetime or largely wholetime appointments should have been made, except in cases in which the consultant is engaged for part of his time in some form of original research. There is an enormous field and an urgent need for both clinical and technical research, and of making original contributions to Physical Medicine, and in selected instances, consultants should be freed for this purpose. Indeed, it is unfortunate that the universities, with their unique facilities in the basic sciences, have so far refrained from making academic appointments in this field. But, apart from considerations of research, every healthy speciality should contain a fair proportion of consultants engaged part-time in competitive practice, in order to acquire a personal contact with the patient, his relatives and his general practitioner. In this way the consultant will be better able to teach, because his knowledge is more practical and less academic. The full-time or nearly full-time specialist is apt to become engulfed in administration, and in the endless committee meetings of modern hospital practice. He thus tends to lose what is, in this speciality, of particular—indeed, of vital—importance, the humanities of medicine.

CONCLUSION

I have attempted to record the history of our Speciality from its inception to the present time; to trace its evolution; to point out the faith which has guided men approaching it from many different angles, to weld it into one concrete whole; and to point out the difficulties which have been overcome, and those which yet remain to be solved.

Section of Surgery

President—DAVID H. PATEY, M.S., F.R.C.S.
in the Chair

[*May* 20, 1952]

HALSTED CENTENARY MEETING

THIS meeting was organized by the Section of Surgery to honour the memory of one of the greatest of American Surgeons—William Stewart Halsted. Halsted was one of that select band of surgeons who exercised a fundamental influence on the advance of surgery not only in their own country but all over the world. That this meeting was held in London is also a symbol of the ties of science, art and friendship which unite British and American surgery.

The meeting opened with the presentation by Lord Webb-Johnson of the Honorary Fellowship of the Royal Society of Medicine to Dr. Alfred Blalock, the present successor of Halsted in the Chair of Surgery at The Johns Hopkins University School of Medicine. Dr. Blalock then presented to Sir Cecil Wakeley, President of the Royal College of Surgeons of England, a copy in oils of the well-known portrait of Halsted, which will hang in the Royal College of Surgeons. Later, Dr. S. J. Crowe presented to Lord Webb-Johnson Halsted's personal walking-stick for the historical collection of the Royal Society of Medicine. A message of greetings and good wishes for the success of the meeting from the American Ambassador, Mr. Walter S. Gifford, was read by Lord Webb-Johnson.

William Stewart Halsted and His Influence on Surgery

By ALFRED BLALOCK, M.D.

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I WELCOME the opportunity of participating in this occasion on which the Surgical Section of the Royal Society of Medicine demonstrates its friendship for its colleagues across the Atlantic by honouring the memory of William Stewart Halsted, regarded by many of us as the most eminent surgeon America has produced. It should be made clear that I am not one of Dr. Halsted's official family, that I cannot claim the distinction of having had much of my training under him, and that I am simply a representative of the department of surgery which he created and developed and headed for the first thirty years of its existence. Interestingly enough, the position which he has held had been declined by Macewen of Glasgow. It is not surprising that a surgeon from the British Isles should have been offered the position since a common language, kinship, and scientific inter-course combined to render the influence emanating from these Isles the dominant one in the development of American medicine up to 1870. The period of the most profound influence of English upon American surgery was from 1760 to 1830 and was due to the eminence of London surgeons, including John Hunter, Percival Pott, John Abernethy and Sir Astley Cooper. For example, the list of American pupils of Sir Astley includes John Collins Warren, James Jackson, Valentine Seaman, Valentine Mott, Edward Delafield, and John Wagner. During this period the scientific interest of surgeons was largely in normal and pathological anatomy. Following a time in which the work of Pasteur, Lister and others predominated, the scene of greatest scientific endeavour shifted to the German-speaking countries, and aspiring young American medical scientists began to visit and to work on the Continent. This period is marked especially by the establishment of good laboratories and by rapid developments in pathology, bacteriology and chemistry.

There were about twenty university medical schools in Germany in 1870 whereas there were many medical schools, but none of university rank, in the United States. It is true that Michigan, Pennsylvania and Harvard Universities had effected distinct improvements in their medical schools but these still left much to be desired. At about this time Mr. Johns Hopkins, a Baltimore merchant and banker, left his fortune for the establishment of a university and a hospital, with separate boards

of trustees. It is of interest that he or his advisers had the wisdom to include the following directive: "In all your arrangements in relation to this hospital, you will bear constantly in mind that it is my wish and purpose that the institution should ultimately form a part of the medical school of that university for which I have made ample provision by my will." It is to be emphasized that at that time in 1870 most of the medical schools in America were proprietary in nature.

It is doubtful whether the will of a philanthropist was ever carried out by his trustees more exactly or more intelligently than was that of Mr. Hopkins. In the first place, they chose Mr. Daniel Coit Gilman as president of the university and as first director of the hospital. He established the first important school of postgraduate study in the United States. Furthermore, President Gilman and the trustees had the wisdom to choose Dr. John S. Billings as adviser and Dr. William H. Welch as dean and professor of pathology. Before returning to Dr. Halsted, who was a close friend and an appointee of Dr. Welch, I should like to refer briefly to the pre-Hopkins career of Dr. Welch, who became generally known as the Dean of American Medicine.

Dr. Welch was graduated from the College of Physicians and Surgeons in New York in 1875. He had attended two-yearly courses of lectures of five months' duration each, the lectures being repeated each year, and had had three years' experience as assistant to his father, a physician. Following graduation, he went to Germany for work in histology, pathology and physiology, a part of the time being devoted to work on pulmonary œdema in the laboratory of Julius Cohnheim. More important than his original work was the perspective which he gained of medical education. He became convinced that close association between the laboratories of the medical school and the wards of the hospital was necessary in the teaching and advancement of medicine. He realized the defects in American medical institutions and resolved to correct them if given the opportunity. He returned to New York and established a laboratory of pathology at Bellevue Hospital. The opportunity for which he had longed while working in Germany came in 1884 with the development of plans for the Hopkins Medical School and Hospital and his selection as Dean and Professor of Pathology. He was destined to be the leader of a remarkable group of medical scientists and he became the most influential and revered man in American medicine.

We turn now to Dr. Welch's close friend and protégé, William Stewart Halsted, who was born in New York City in 1852. His parents were descended from a line of cultured and gentle people of English stock who for several generations had been distinguished in the social, commercial and philanthropic life of New York. His career at Yale University, at which he was graduated in arts in 1874, was distinguished only by his athletic prowess. This is in marked contrast to his pre-eminent record at the College of Physicians and Surgeons (Columbia University) from which he was graduated in 1877. It was while serving an internship in New York that he first met Dr. Welch, who had just returned from Europe. In the fall of 1878 Dr. Halsted went to Europe for two years of study. The greater part of his time was spent in Vienna, Leipzig, Würzburg and Halle, and he was probably most influenced by the teaching of Volkmann, Billroth, Thiersch, Bergmann and Mikulicz. These were the golden days in medicine. Bacteriology was dawning, embryology and histology were developing, pathological anatomy was being studied with great vigour and the teachings of Lister were being accepted in Europe. These two years spent in Europe had a profound influence on the future of this young man who was aware of the many deficiencies in the medical schools and hospitals of America. Among other benefits this period of freedom from clinical duties allowed time for study and thought.

Following his return to New York in 1880, where he was to remain for six years, Halsted exhibited the greatest activity of his long career. He was well trained and socially prominent, he had energy and imagination, and his exceptional merit and ability were quickly recognized by his appointment as visiting surgeon to many hospitals in New York City. He developed the most popular private "quiz" class in New York for medical students. Despite the long hours spent in teaching and the care of patients, he found time for investigative work. Probably his most important contribution of this period was that on cocaine, the anæsthetic properties of which had been discovered by Koller. Halsted and his associates demonstrated the superiority of endodermal as distinguished from the hypodermal injection method, the efficacy of dilute solutions of the analgesic drug when administered in large quantities, the prolongation of effect of the drug when the circulation of the part is reduced, and most important, the fact that sectional infiltration of a sensory or mixed nerve dulls sensation throughout the peripheral distribution of the nerve—the neuroregional method of anæsthesia. This important work on cocaine led to a temporary cessation of Dr. Halsted's activities for he and some of his associates, knowing nothing of the habit-forming characteristic of the drug, became addicted to its use. His disability continued for more than a year, most of which time he spent in a hospital in Rhode Island. On leaving the hospital he joined Dr. Welch in Baltimore and the two of them lived together for a time. He had, however, to return to the hospital in Rhode Island for an additional short period of treatment. All evidence indicates that he overcame his addiction at that time.

Whereas, before coming to Baltimore, Osler had been offered and accepted the position as Head of the Department of Medicine and Kelly that in Gynæcology in the Johns Hopkins Medical School and Hospital, Halsted joined his close friend, Dr. Welch, without having received such an attractive offer. In fact, as stated previously, the professorship of surgery was offered to Sir William Macewen

of Glasgow. At any rate, when the hospital opened in 1889, Halsted was appointed for one year on an "acting" basis as surgeon to the hospital at a salary of one thousand dollars per annum. It is likely that the question of his health accounted for the caution on the part of the trustees and faculty.

When Dr. Halsted came to Baltimore in December, 1886, two years before the opening of the hospital and six years before the first class was admitted to the medical school, he found a brilliant group of young men working with Dr. Welch in the laboratory of pathology. Thus for a second time in his career, the first having been in Germany and Austria, Dr. Halsted had the opportunity and leisure to work in the basic sciences. Under the watchful care and inspiring leadership of Dr. Welch, he rapidly regained his health and confidence in himself, and at the same time had the good fortune to work in close association with the remarkable group in the laboratory including Mall, Councilman, Nuttall, Walter Reed, Abbott, and Simon Flexner. These surroundings were ideal for a person of Dr. Halsted's ability and training. Councilman, associate of Welch, and later Professor of Pathology at Harvard, stated: "Halsted proceeded to work on the basic question of operative procedure and the treatment of wounds. He read all the old papers of Lister which were published in the *Lancet* and carried out all the methods which he gave experimentally on animals, but with a more thorough study of the results than would have been possible when Lister did his work, for in 1886 the science of bacteriology had been developed and reliable methods of work established. The methods of Lister were complicated and involved the use of materials which were difficult to procure. Halsted showed that cultures made of wounds treated after the most meticulous use of the Lister methods showed the presence of bacteria on the surface. This led him to a careful microscopic study of wounds and the realization that care in operating, the exact approximation of surfaces and the avoidance of dead spaces were as important for results as the supposed avoidance of bacteria.... The study of hand disin-fection was no less thorough and this led to the realization that the hands could not be sterilized, and finally to the use of rubber gloves in operating.... It is difficult to think of surgery more carefully conducted than was this experimental surgery of Halsted.... Probably not since the time of John Hunter had the experiment been so fully utilized in the development of surgery." This brief analysis of Halsted's early contributions appears to me to be a particularly good one.

Halsted's work and the continuing good state of his health impressed the faculty and trustees favourably, and he was appointed surgeon-in-chief to the hospital shortly after its opening, and two years later was appointed professor of surgery in the medical school. Dr. Welch said later, "No greater good fortune could have befallen the Johns Hopkins Hospital and later the Medical School than to find Halsted here on the ground and available for the position of Surgeon-in-Chief to the Hospital and Professor of Surgery". Comment has already been made upon the period of relative leisure, free of the care of patients, before the opening of the hospital. The fact that the opening of the hospital preceded that of the medical school by four years was probably of equal significance. The men gathered together by Gilman, Welch and others were all interested in teaching as well as research and they seized upon the opportunity to develop the residency training programme, which has proved to be a superb system of postgraduate medical education. In my opinion the greatest contribution of the Johns Hopkins to American medicine and surgery has been the residency system. Dr. Welch said: "Here Halsted first in America created a genuine school of surgery. He shares with Osler and Kelly the credit of organizing the clinical services according to a plan whereby it was rendered possible for the young resident surgeons and physicians, appointed on indefinite tenure, to be trained during long periods of time for the higher academic and professional careers." Halsted was fully aware of the defects in surgical training in America and he had been favourably impressed with the German system. The two principal features of this latter system were these: (1) A close blending of the work in the basic sciences and in the clinic, and (2) a prolonged postgraduate training in which only the best candidates were retained for a term of several years. The Halsted modification of this German system appears to me to consist in the main in the concentration of responsibility and authority in the resident rather than in the Geheimrat. Details of his plan for "The Training of a Surgeon" are given in his memorable address at Yale in 1904.[1] This system appears to have been particularly suited to a man of Dr. Halsted's taste, temperament and habits. He was more interested in and seemed to have a greater affection for younger men than for those of his own age, save for a few exceptions such as Welch. When away from Baltimore, whether in Europe or at his summer home in North Carolina, he felt that his responsibilities would be carried out by his resident. He did not hesitate to call upon his resident to look after his own private patients and to perform other duties. For example, it is related that Dr. Halsted would work late at night in preparing a clinic for the students only to find on awakening that he was fatigued, and he would

[1] It was in this address that he stated: "Anæsthesia, one of the greatest blessings, is at the same time one of our greatest reproaches, hæmorrhage is still awkwardly checked, and of surgical infection once started we have often little control and then mainly by means of the knife. We have reason to hope that the day will come when hæmorrhage will be controlled by a quicker procedure than the awkward, time-consuming ligature; when infections will be controlled by specific products of the laboratory; and when pain will be prevented by a drug which will have an affinity only for the definite sensory cells which it is desirable it should affect. The first of these may be last and the last first. Let us trust that it may, as Gross expresses it, 'be a long time before the laws of this department of the healing art will be as immutable as those of the Medes and Persians'."

call upon his resident to perform his operations and to give the clinic. The resident always knew of Dr. Halsted's investigative interests, and he never failed to see that patients who fell within this category remained around until "the Professor", as he was called, had the time and inclination to see them. At any rate, the system worked to perfection under Halsted, ably assisted by Finney and others. Dr. Halsted's system is, in fact, the easiest way, within certain limitations, of conducting a teaching service, particularly when worthy candidates are competing for the residency. According to Welch, Halsted often said that he found the greatest satisfaction of his life in the training of surgeons. The results will be commented upon later.

The first fifteen years after the opening of the hospital in 1889 were the most productive of Halsted's life. It was a life totally different from that which he had led in New York and in the pre-cocaine era. He had changed from the busy surgeon with many appointments and acquaintances to the quiet scholar who sought seclusion. He was no longer the rapid spectacular operator, but rather the extremely careful yet not unduly slow one. He continued his studies on intestinal suture, wound healing, and other basic subjects, and at the same time improved or developed operations for hernia, carcinoma of the breast, thyroid disease, aneurysm and various other disorders. His frequent trips to Europe continued, and a real bond of affection and admiration developed between him and Theodore Kocher[1] of Bern. By the end of this fifteen-year period he had trained such men as Cushing, Bloodgood, Mitchell, and others and in all respects had attained remarkable stature for a man who had just passed 50.

Although the later years in Halsted's life were somewhat less productive, he continued to make original contributions and to train residents. Furthermore, he placed outstanding men in the special fields of surgery and stimulated them to develop these fields. Among these were Young in urology, Cushing and Dandy in neurosurgery, Baetjer in radiology, Baer in orthopædics, Crowe in oto-laryngology, and Davis in plastic surgery. Some of Dr. Halsted's associates are shown in Fig. 1.

At the recent Halsted Centennial Celebration in Baltimore, Dr. B. N. Carter, Professor of Surgery at the University of Cincinnati, gave an interesting paper on " The Fruition of Halsted's Concept of Surgical Training", or the Halsted Genealogical Tree. Carter was assistant resident under Halsted and resident under Heuer and Reid at Cincinnati. This survey by Carter consists of an analysis of the attainments of Dr. Halsted's resident surgeons, his assistant resident surgeons, and the second generation of resident surgeons trained by his resident surgeons. In view of the fact that Dr. Halsted's clinical service was not large, that the number of house officers was small, and that the senior resident retained his position as long as four and a half years in one case, the successful operation of the system has been more widespread than one would have anticipated. For reasons of brevity I will repeat only Carter's figures relating to the entire group of 238, which comprises Halsted's resident surgeons, 17 in number, assistant resident surgeons, 55, and the second generation of resident surgeons, 166. Among this group of 238 there have been:

37 Professors	17 Assistant Professors
14 Clinical Professors	16 Clinical Assistant Professors
18 Associate Professors	23 Instructors
14 Clinical Associate Professors	99 Private Practitioners of Surgery

It is to be borne in mind that those who were students under Halsted or served only as internes are excluded from the survey. It is evident from Carter's analysis that Dr. Halsted succeeded in training a group of surgeons who were so imbued with his principles of thought and action that they, in turn, are handing down these principles successfully to their assistants and students. Carter concludes by stating: "Were the Professor alive today, the group which would intrigue him most would be those 166 men who represent the second generation of resident surgeons. For they are the ones to whom it is now given to carry his torch and to create a third and larger generation of surgeons, who will also be imbued with the principles of their remarkable progenitor, William S. Halsted." According to W. M. Firor, recent chairman of the American Board of Surgery, the Johns Hopkins Hospital in 1901 had the only fully developed residency system in surgery in the United States; in 1941 there were 30 residencies patterned after it, and in 1951 the number had increased to 130. It is only natural that some of these should be better than others. Unfortunately it is not fully appreciated by the heads of some of these systems that there should be a gradual advancement in responsibility during the years as assistant resident and that the resident should have the major responsibility for the diagnosis and treatment of ward patients, including the seriously ill.

Before concluding, I should like to try to comply with the suggestion of Mr. Patey, President of the Surgical Section of this Society and your admirable representative at the Halsted Celebration in Baltimore, that I attempt to trace the continuing influence of Halsted in the development of cardio-vascular surgery. I find this difficult to do except in terms of his basic contributions to the entire field of surgery. These contributions relate to wound healing in general as well as to the surgery of individual

[1] Dr. Halsted stated: " Many times during the past twenty years I have stood by the side of Professor Kocher at the operating table, enjoying the rare experience of feeling in quite complete harmony with the methods of the operator, and it is a pleasure to give expression to the sense of great obligation which I feel to this gifted master of his Art and Science."

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Fig. 1.

structures such as blood vessels. Except for these basic studies, Halsted's contributions to vascular surgery dealt mainly with the treatment of aneurysm. Following in the footsteps of John Hunter, Sir Astley Cooper and others, he was particularly interested in the development of methods that would produce a gradual occlusion of large arteries and thereby stimulate the development of collateral arterial pathways. He enunciated the important principle that the more centrally an arterial ligature is placed, the less is the chance of gangrene. It appears to me that his choice of the metal band for the purpose of constricting large arteries was a poor one because its use so frequently resulted in erosion of the artery and fatal hæmorrhage. It is true that some important physiopathological observations resulted from these experimental and clinical studies, such as the healing process in arteries subjected to varying degrees of constriction, and the dilatation of arteries distal to a stenotic area. Furthermore, it certainly is true that he stimulated the subsequent work of Holman, Reid, Reichert and others on arteriovenous fistula, lymphœdema of the extremities and other subjects. Dr. Halsted was the first to ligate successfully the first portion of the left subclavian artery in the treatment of an aneurysm of this vessel (1892), and he stated later that he had "failed to obtain any evidence that gangrene has been caused by the uncomplicated ligation or ligations of either subclavian artery". This observation was reassuring to me and my associates when we decided to use a subclavian artery in creating an artificial ductus in the treatment of pulmonary stenosis, although at that time the subclavian artery had been ligated in only a few patients. I must insist, however, that Halsted's contributions to vascular surgery were mainly the enunciation of important general principles, such as the gentle handling of tissues, good hæmostasis, transfixion of individual vessels, rigid asepsis, the use of small suture material, fine artery forceps, rubber gloves, rubber tissue drains, and other refinements. These general principles, added to those of Lister and others and strongly reinforced by better anæsthesia, the use of antibiotics, and blood banks, have aided greatly in the advances in cardiovascular surgery.

Time will not permit me to deal with these recent advances in any detail. Perhaps I might mention the work on the treatment of aneurysms by wiring and electrothermic coagulation plus constriction of the artery proximal to the aneurysm; improvements in methods for treating varicose veins and ulcers and arteriovenous fistulæ; the extensive use of sympathectomy in the treatment of hypertension and peripheral vascular disease; the surgical closure of the patent ductus arteriosus; the treatment of coarctation of the aorta by excision and end-to-end anastomosis or by the use of autografts or homografts for bridging the defect; the treatment of the tetralogy of Fallot by making an artificial ductus or by a direct attack on the infundibular stenosis; the treatment of "pure" pulmonary valvular stenosis by division of the valve; the treatment of mitral stenosis by commissurotomy; the employment of decortication of the heart, particularly the ventricles, in the treatment of constrictive pericarditis; the creation of interauricular defects in the treatment of tricuspid atresia and transposition of the aorta and the pulmonary artery; the use of endarteriectomy or of blood-vessel grafts in the treatment of obliterative disease of large arteries, the employment of portal-caval shunts in the treatment of bleeding œsophageal varices as well as the treatment of many other disorders. The principles enunciated by John Hunter, by Lister, by Halsted, and by others are invaluable in the advances in cardiovascular as well as in all branches of surgery. Therefore, as a general surgeon, I am happy to have the opportunity to stress the important role that advances in basic general principles have played in the progress of all branches of surgery, and particularly that branch in which Dr. Halsted had a special interest, namely, cardiovascular surgery. He stated on a number of occasions that "the surgeon's method of dealing with the blood vessels is a criterion of his proficiency in his art", and he urged his associates to become expert in the anastomosis of arteries. Many of those who came directly or indirectly under his influence and teaching have done much in widening the scope of vascular surgery.

Some say, and I think with truth, that the stature of Halsted has grown with the passing of years. Perhaps the same might be said of Harvey, John Hunter and Lister and of most truly great men. It should not be thought, however, that Halsted was not appreciated in his lifetime. Your renowned Royal College of Surgeons at its centenary in 1900 conferred an honorary fellowship on him, and he received a similar honour in Edinburgh in 1905. Professor Leriche published a beautiful tribute to him in 1914, eight years before his death. John Chalmers Da Costa, Samuel D. Gross Professor of Surgery at Jefferson Medical College in Philadelphia, in 1919 dedicated his textbook of surgery to Halsted in these words: "This book is dedicated to the chief surgeon and inspiration of one of the greatest, most progressive and most influential surgical clinics in the world. A clinic from which come important facts, real ideas and brilliant men. To the operator, the teacher, the investigator and the surgical philosopher. To Dr. William Stewart Halsted, the distinguished Professor of Surgery in the Johns Hopkins University." The National Dental Association of America gave him a gold medal in recognition of his work on neuroregional anæsthesia and he was elected to membership in the National Academy of Sciences. He must have been very happy in his accomplishments when his life ended in 1922. The president of the Johns Hopkins University, Frank J. Goodnow, stated at that time, "It is only characteristic of the man that when he died he not only left the lustre of a name that will always be honoured by Hopkins men, but as well made the University his residuary legatee in order to enable it more adequately to carry on the work which he had so auspiciously

begun, namely, research in surgery". Recipients of the Halsted Fellowship have made significant contributions to surgery.

I should like at this point to quote from two tributes to Dr. Halsted that appeared shortly after his death. The first is by his brilliant pupil, Harvey Cushing, who wrote: "A man of unique personality, shy, something of a recluse, fastidious in his tastes and in his friendships, an aristocrat in his breeding, scholarly in his habits, the victim for many years of indifferent health, he nevertheless was one of the few American surgeons who may be considered to have established a school of surgery, comparable, in a sense, to the school of Billroth in Vienna. He had few of the qualities supposed to accompany what the world regards as a successful surgeon. Overmodest about his work, indifferent to matters of priority, caring little for the gregarious gatherings of medical men, unassuming, having little interest in private practice, he spent his medical life avoiding patients—even students, when this was possible—and, when health permitted, working in clinic and laboratory at the solution of a succession of problems which aroused his interest. He had that rare form of imagination which sees problems, and the technical ability combined with persistence which enabled him to attack them with promise of a successful issue." The second tribute is by his close friend, Dr. Rudolph Matas, who said: "Modest, self-repressed, shunning the limelight of publicity, he never obtruded his personality or exploited his deeds and only referred to himself in the most impersonal way, allowing the facts always to speak for themselves. Though shy and reserved and undemonstrative, he delighted in the company of his pupils, immediate associates and a few chosen friends. With these, he was expansive, and the glow and warmth of his friendship rapidly dissipated whatever restraint might first have been imposed by his punctilious politeness and adhesion to conventional formalities. To those who were privileged to bask in the sunshine of his friendship, the true nature of the man was revealed in all its splendour." The tributes of Leriche, Heuer, MacCallum, Finney, Chesney, and others supplement and corroborate these statements.

Were Dr. Halsted alive, I know he would be deeply grateful that the Royal Society of Medicine of London and the Academy of Surgery of Paris sent representatives to the recent celebration in Baltimore in the persons of Mr. David Patey and Professor René Leriche. He would be even more pleased that the Surgical Section of this august body should take cognizance of his accomplishments. Perhaps you do so in part because his inquiring turn of mind reminds one of John Hunter, whom he admired above all the surgeons of the past and for whom he named the surgical experimental laboratory at the Hopkins. Further, you may have been influenced by the similarities between Lister and Halsted, particularly their self-effacing modesty and their conviction that "the operating room is a laboratory for the surgeon". As a representative of my school and of American surgery, I wish to thank all of you most heartily, and particularly Lord Webb-Johnson and Mr. Patey, for this occasion, which your many friends in the United States and in Canada sincerely appreciate.

The Paradox of William Stewart Halsted

By W. R. BETT, M.R.C.S., L.R.C.P., F.R.S.L.

IN the story of Halsted's life there are two entirely different individuals. In his younger days in New York City he had been a keen athlete; a prodigious worker, quenchless in his enthusiasm for life in all its manifold activities; a bold, rapid, spectacular surgeon; and one of the most popular clinical teachers of his day, who caught and held the most eager attention. When he went to Baltimore, the sudden change in his personality intrigued and baffled the world. He had become a recluse, aloof, shy, fastidious alike in dress and in manner. Avoiding patients, students, and the administrative burdens of office—so notoriously detrimental to the best work, instead he sought leisure for study and for reflection. He had become a meticulous, infinitely safe surgeon who revelled in all the details of operative technique. The cheerful extrovert now shrivelled men with his biting sarcasm. Few were at ease in his presence. What was this paradox of the greatest surgical philosopher America has ever produced, who in his quiet and sometimes halting way created in his country a school, a philosophy, and a tradition of surgery, which Harvey Cushing has compared with those of Billroth in Vienna? In his New York period tragedy had overtaken Halsted and had made him a cocaine addict. The shadow of this tragedy he was to see all his life, and all his life men were to see this shadow in his face.

Soon after Karl Koller had communicated his researches on the anæsthetic properties of cocaine, when applied to the cornea, to the German Ophthalmological Society meeting in Heidelberg in September 1884, Halsted and two assistants, Richard J. Hall and Frank Hartley, at the Roosevelt Hospital, New York, began to experiment with this drug on themselves. Within a year of Koller's announcement Halsted had created neuroregional anæsthesia, reporting the successful use of cocaine in more than one thousand surgical operations. He little suspected at the time the diabolic effects which the drug can produce. The three experimenters did not escape. All became cocaine addicts. The two assistants died. Halsted was rescued by William H. Welch, who put him away for a year in a hospital in Providence, Rhode Island, and afterwards took him to Baltimore. Though Halsted had to return to hospital for a second period of treatment, one is probably justified in stating that

he subsequently succeeded, through strength of will and self-discipline, in overcoming his addiction to cocaine.

One is probably also justified in stating that, though he climbed to the pinnacle of professional eminence and achieved both national and international fame in the world of surgery, the evil drug had forever left its mark on the man.

There surely also is an element of paradox in one of the original discoveries attributed to this great surgeon: the introduction of rubber gloves. Halsted's sole purpose had been to protect the hands of his glamorous theatre nurse, whose skin was hypersensitive to the strong mercuric chloride solutions then extensively used, rather than to eliminate the surgeon as a source of sepsis. Everyone in Baltimore in those early days made fun of gloves. Halsted himself was the last to wear them. All his life, by the by, he expressed surprise that the nurse in question should have married one so unworthy of her as himself, which, as Dr. Welch always insisted, is the correct attitude for a husband to adopt. Happily married, he passes out of the chapter, while I conclude on a personal note. In my suburban and incredibly prosaic life it is a veritable thrill to find myself appearing on the same programme as that king of vascular surgery, Dr. Alfred Blalock. Among Dr. Blalock's many wise and shrewd sayings there is one that comes home vividly to me in relation to the famous man whom we are assembled to praise and who year by year seems to grow in intellectual stature: "It is not necessary to live too closely to great men. It is better to admire them from afar."

Personal Recollections of Doctor Halsted

By S. J. CROWE, M.D.

Otorhinolaryngologist in Chief, Johns Hopkins Hospital, Baltimore, U.S.A.

IT was by the purest chance that I came to know Doctor Halsted the man, and not merely the surgeon. To make this clear I will have to tell the story of the sick horse. At the end of my first year in the Johns Hopkins Medical School, June 1905, I asked two friends to join me on a camping trip in the North Carolina mountains. We hired a wagon and two percheron horses, and for several weeks drove aimlessly through the mountains with no maps or guides of any kind. At night we slept in the waggon and tied the horses to trees. This was an unusually cold and rainy summer and one morning when I went to feed the horses I found one of them standing with front legs far apart, head hanging and so stiff that it was difficult to get him to move.

Here I was, a relatively penniless medical student, deep in the mountains far from any railroad or town, with a hired horse that looked as if he would never walk again. Without waiting for breakfast, a shave or a wash I mounted the well horse and, moving at a snail's pace, led the other down the road in search of a barn or some advice about what to do for a sick horse. Finally I came to a side road and saw a log cabin in the distance. An old mountaineer was sitting on the porch. He agreed with me that this horse should be under cover and should be seen by a doctor, but he had no other suggestions. It was quite evident that his stable was far too small to house such a huge animal. I was becoming more worried every minute. To my intense irritation the man seemed far more interested in learning where I came from, where I was going and why I was driving through the mountains than he was in helping me with the horse. It finally dawned on me that he suspected I was a revenue agent hunting for illicit stills. I answered his questions as patiently as I could and finally told him I was a medical student at the Johns Hopkins in Baltimore and was here on a vacation. Immediately he became more friendly and informed me that Doctor Halsted lived about two miles up the road, that he also came from Baltimore and that his barn was the only one in these mountains large enough for a horse of this size.

Leaving the sick animal standing in the road I soon arrived at Doctor Halsted's estate, High Hampton, which consisted of 3,000 acres of woodland, two small houses, a beautifully kept lawn, row upon row of the most gorgeous dahlias, and a large barn. It was the barn that interested me most. I subsequently learned from Doctor Halsted that he had been collecting rare and beautiful varieties of dahlias for many years. This garden was his chief delight. My knock on the door of the larger of the two houses was answered by a maid in a dainty white uniform and cap. She said Doctor Halsted was out walking on the place. I sat on the porch feeling very uncomfortable because of my dirty clothes and dishevelled appearance. This feeling was accentuated when Doctor Halsted appeared about an hour later in spotless white flannels. I told him who I was and why I had called on him. Instead of sending his stable man with me, he immediately had his buggy brought around and insisted that we drive down and look at the sick horse. That impressed me as being exceedingly kind and polite to a first-year medical student he had never seen before, but what impressed me most was the care and patience with which Doctor Halsted examined the horse after we got there. Disregarding the muddy road he got out of the buggy and for fully 30 minutes palpated every joint and muscle, and observed very minutely the horse's gait, as I walked him up and down the road.

All this time the mountaineer and his family leaned on the fence and looked. They were not surprised at what they saw. For years the natives had been accustomed to call on Doctor or Mrs. Halsted when their cow, their horse or any member of their family was sick. The Halsteds never

refused, because doctors were few and far between in those mountains forty-seven years ago. Doctor Halsted had always been a student of anatomy, and the number of sick and injured animals he was asked to see during his vacation period in the mountains had stimulated in him an interest in veterinary medicine. He had acquired text books and the necessary instruments, and each year had become more and more interested in the treatment of sick and injured horses and dogs. Mrs. Halsted, who had spent most of her life on a plantation and was accustomed to seeing animals operated on and treated with the crude methods of the natives was greatly amused at some of the Professor's exploits in the field of veterinary medicine. At any rate Doctor Halsted diagnosed my horse's disability as muscular rheumatism. It is true he advocated bandaging the horse from head to foot with oiled silk, which of course was not available, so he had me go to the country store and buy oilcloth such as is used on kitchen tables. After a week in a box stall, rubbed daily with liniment and bandaged with strips of oilcloth, the horse recovered and I went on my way.

That week at High Hampton initiated a change in my attitude toward medicine. Every morning Doctor Halsted examined the horse and then, sometimes accompanied by Mrs. Halsted, we would walk through the dahlia garden inspecting the flowers for insects, or sit in chairs on the lawn and talk. Looking back on those days I realize what this chance meeting with Doctor Halsted has meant to me. He pictured medicine as a living, growing, constantly changing and most fascinating study. To me this was a new concept and when I returned to the medical school a few weeks later, it was with enthusiasm and joy.

Doctor Halsted was a most gracious and charming host, always full of conversation, sparkling with wit, and interested in a thousand things. Others have pictured him as shy and aloof. This is not the Doctor Halsted I knew. He and Mrs. Halsted when at High Hampton delighted in driving along the mountain roads with Nip and Tuck, the older dachshunds in the buggy with them, while Otto, Mädchen and the setters ran along beside them. The dachshund was Doctor Halsted's favourite dog. In a letter dated January 3, 1903, to the young son of one of the University Professors he said: "You must come and see our new dachshund; she is a great beauty, a brunette with a lovely complexion and all the graces of her sex." When, on these drives they met any of the mountain people Doctor Halsted usually stopped and talked, often I am afraid in language quite beyond their comprehension. He was extremely popular, however, having treated so many of them for minor injuries and sending the more seriously ill to the Johns Hopkins Hospital at his own expense. The whole countryside rang with praise at the miraculous thing he did for one patient. This man was working with a band saw and had several of his fingers cut off. The bleeding was controlled with a handkerchief tied around his arm and tightened with a stick. The severed fingers were wrapped in a piece of newspaper and the man was brought to Doctor Halsted with the request that he sew them on. This he did, and the hand healed without infection or post-operative œdema. This was many years prior to his experiments showing that wound infection, and not solely blocking of lymphatics and veins, was the cause of swelling of the arm following radical operation for carcinoma of the breast.

During the next seventeen years it was my privilege to see much of Doctor and Mrs. Halsted in their house at 1201 Eutaw Place. There I learned the story of how rubber gloves came to be used in the operating room. Mrs. Halsted was Miss Caroline Hampton the niece of Wade Hampton of South Carolina, who before the Civil War was the largest cotton grower in America. During the war he was elevated to the rank of Lieutenant-General and was on General Lee's Staff. Her father, also an officer, was killed in the battle of Brandy Station in Virginia. After the war the Hamptons were desperately poor and Miss Caroline determined to become a nurse. She graduated at the New York Hospital in 1889 and was appointed head nurse in the surgical division of the Johns Hopkins Hospital when it was formally opened in May of that same year. In those days and for approximately twenty-five years thereafter, it was the custom on the surgical service to scrub the hands and arms with soap and water, dip them into a saturated aqueous solution of potassium permanganate, then of oxalic acid, and finally soak them for ten minutes in a long, narrow basin filled with a 1 : 1,000 mercuric chloride solution. This technique produced such a marked dermatitis on Miss Hampton's arms and hands that Doctor Halsted asked the Goodyear Rubber Company to make as an experiment two pairs of thin rubber gloves with gauntlets. These gloves proved so satisfactory that additional gloves were ordered. Within a few months not only the nurse, but also the assistant who threaded the needles and passed the instruments began to wear gloves. For several years after this, the operator and first assistant wore gloves only for operations on joints or the central nervous system. It was realized that gloves improved the aseptic technique, but for a long time it was thought that they impaired the sensitivity of touch. I have seen Doctor Halsted and senior members of the staff remove their gloves to palpate a nodule in the breast or to feel for stones in the bile passages. The younger members of the staff, however, grew up with gloves and when they became Residents they rarely if ever removed a glove to improve the sense of touch.

Miss Hampton was head nurse in the operating room for only one year. She and Doctor Halsted were married in June 1890 and went to the Hampton family hunting lodge in the North Carolina mountains for their wedding trip. This is the property that Doctor Halsted later bought and called High Hampton. Both Doctor and Mrs. Halsted had a keen sense of humour, and one day over a cup of tea they told this amusing incident of their wedding trip. Doctor Halsted had been born and

bred in New York City and occasionally had ridden a horse in Central Park, but Mrs. Halsted was an expert and enthusiastic horsewoman. She was anxious to show him the place, and a day or so after their arrival at the hunting lodge the two of them were riding along a trail in the woods when Mrs. Halsted suddenly pulled up her horse and pointing with her riding crop exclaimed in the greatest delight: "William, there is a rattlesnake, get down and kill it." Looking at me with a twinkle in his eye Doctor Halsted said: "There I was alone in the mountains with this comparatively strange woman, and she wanted me to get off my horse and kill a rattlesnake. She was terribly disgusted when I refused."

They were a devoted pair, although entirely different. His chief interest was surgery, research and his medical books. He was not interested in poetry or general literature. His recreations in Baltimore were the collection of antique and beautiful furniture, rugs, china and silver, which filled his house on Eutaw Place and at High Hampton. He liked the theatre, but cared little for art or music. When in North Carolina his chief interests were dahlias, the study of comparative anatomy and diseases of animals, and astronomy. He had a beautiful reflecting telescope and showed me many charts he had made of the moon, the planets and their satellites and some of the fixed stars. Mrs. Halsted's chief interest in life was to make everything comfortable and pleasant for Doctor Halsted, and to interfere as little as possible with his work. Their Baltimore home was a large three-storey brick house. Mrs. Halsted's quarters and the guest rooms were on the third floor. He and his books occupied the second floor. The house was largely heated by open wood fires. Doctor Halsted liked only hickory and white oak. He also was very fond of fresh guinea eggs for breakfast, said they had a gamey taste. On many occasions Mrs. Crowe and I have driven Mrs. Halsted far into the country around Baltimore searching for just the right kind of firewood and the right kind of eggs for the Professor. Mrs. Halsted always went to High Hampton several weeks before the date of Doctor Halsted's arrival in order to supervise the planting of the dahlias and the vegetable garden, and to get the lawn in perfect condition. She was a very practical and efficient woman. In a letter written in 1914, she mentions that she has sold a calf to one of the mountain women and received in payment a stove, four pairs of knitted wool socks, $1.10 and half a dozen eggs. The socks were sent to the soldiers.

Doctor Halsted's study was on the second floor adjoining his bedroom. A wood fire always burned in this room. It was filled with the books and journals he was using at the time. His library was on the first floor, but was used only to house the books. He rarely went out, but often worked far into the night on a paper he was writing, or in preparation for a clinic or ward rounds the following day. He consulted all the literature, both ancient and modern, that bore on the condition to be brought before the class. In thus preparing for a clinic he would sometimes become so engrossed in making innumerable notes about new ideas for diagnosis, treatment, or some research project that he lost all count of time. The following day he would be so fatigued that he would call his resident at the hospital and ask him to take over the class and the operations scheduled for that day. When Doctor Halsted did give a clinic, many of the questions he asked the students were far over their head, and often left the house officers without an answer. This method of teaching aroused many of us to think and to search more deeply for the answers than we otherwise might have done.

Few of us realized at the time how much we gained from association with this man. He was tireless in tracking down every bit of information that would help to solve a puzzling problem. Also he took every opportunity to insist to students and house officers that the work of others always should be recognized and given due credit. Never would he publish a clinical report or experimental study until he was sure that the fundamental facts were correct. He often said that all men are fallible in their interpretation of the facts they present, but the facts themselves must be correct. That was his creed. His depth of feeling, loyalty and devotion to his friends are shown in a letter to Mrs. Henry A. Rowland, the widow of the distinguished Professor of Physics at the Johns Hopkins University.

1201 Eutaw Place

Dear Mrs. Rowland,
    I have only this moment heard of your appalling sorrow. If there is anything in the world that I can do please, please tell me. Every moment of my time is at your disposal.

With deepest sympathy and affection
I am,
Faithfully yours,

Tuesday.

At the conclusion of his Address Doctor Crowe presented Doctor Halsted's favourite walking-stick to the Royal Society of Medicine saying that shortly after the death of Doctor Halsted he was given this cane by Mrs. Halsted. It was Doctor Halsted's favourite stick. He used it in Baltimore, on his trips abroad and especially when at his North Carolina estate during the summer.

Doctor Crowe had treasured this cane all these years and it gave him great pleasure to present it through Lord Webb-Johnson to the Royal Society of Medicine.

Further Observations on the Internal Mammary Lymph Chain in Carcinoma of the Breast

By R. S. HANDLEY, O.B.E., F.R.C.S.

I am deeply sensible of the honour which the Council of this Section have done me in asking me to speak this evening for we pay homage, not only to a great American surgeon, but to one who has left a permanent mark on world surgery. I suspect that I have been asked to speak for two reasons. The first is my name, which, thanks to my father's work, is one which is sometimes coupled with Halsted's in discussions on the treatment of breast carcinoma. And the second is that, in collaboration with my colleague in pathology, Dr. A. C. Thackray, I have been able to follow up a thought of Halsted's, a thought which he himself did not pursue. Here it is:

> "Dr. H. W. Cushing, my house surgeon, has in three instances cleared out
> the anterior mediastinum on one side for recurrent cancer. It is very likely,
> I think, that we shall, in the near future, remove the mediastinal contents
> at some of our primary operations."
>
> <div align="right">(W. S. Halsted. Annals of Surgery, 1898.)</div>

I must confess that Dr. Thackray and I drew our inspiration, not direct from Halsted, but from a clinical paper by my father in 1927, in which he argued that the internal mammary lymphatic chain was invaded in carcinoma of the breast much more often than was thought. It seemed to us that the ideas he formulated ought to be tested by the microscope on a quantitative basis, and we therefore started to do biopsies on the internal mammary chain at the end of 1946.

[The speaker then showed slides to illustrate the anatomy of the internal mammary chain and his method of biopsy.]

I have now done biopsies of the internal mammary lymphatic nodes in 119 cases of breast carcinoma. The procedure has been done, not as a part of therapy, but as a pathological reconnaissance. The second space has most often been explored, but latterly we have usually looked into two or more spaces.

The results of this extremely simple manœuvre are as follows:

TABLE I.—INVASION OF INTERNAL MAMMARY LYMPH NODES. ANALYSIS OF 119 CASES

	Site of primary growth		
	Inner half of breast	Outer half of breast	Total
Total number of cases	45 (38%)	74 (62%)	119
All nodes free	10 (9%)	28 (24%)	38 (33%)
Axillary nodes only invaded	8 (7%)	32 (27%)	40 (34%)
Internal mammary nodes only invaded	3 (3%)	1 (1%)	4 (3%)
Both axillary and internal mammary nodes invaded ..	24 (20%)	13 (10%)	37 (31%)

Table I shows that 34% of these growths had metastasized to the internal mammary chain, and where a radical mastectomy was done, it had failed as a curative operation before it started. If we consider the 45 growths in the inner half of the breast, 27 (or 60% of them) had metastasized to the internal mammary chain, whereas of the 74 tumours in the outer half of the breast only 14 (or 19% of the 74) had so metastasized.

Table II shows my first hundred cases analysed by quadrants.

TABLE II.—100 CARCINOMAS OF BREAST

	Site of Primary Growth in Breast by Quadrants					
	Upper inner	Lower inner	Central	Upper outer	Lower outer	Total
Total	20	7	12	50	11	100
All nodes free	8	2	0	19	4	33
Axillary nodes only invaded	3	1	3	21	6	34
Internal mammary nodes only invaded ..	1	0	2	1	0	4
Both internal mammary and axillary nodes invaded	8	4	7	9	1	29

The middle column shows that those growths situated behind the nipple included with inner half tumours in Table I had all metastasized to lymph nodes, 9 of them involving the internal mammary chain.

Nobody yet knows what is the importance of the internal mammary chain. Deposits in the internal mammary chain are often extremely small and it has been maintained that they are too small to matter much. This argument reminds me of the famous excuse given by the young lady who had a baby when she ought not to have done—that it was only a very little one. Some have thought that the tissues of the anterior mediastinum had special powers of resisting the spread of carcinoma. I have

found no evidence to support this view. Yet others have thought that my cases showed an unusually large number of inner-quadrant primary growth, but in fact the quadrant distribution of my first 100 cases corresponds closely to Truscott's very large series. A further objection has been that clinical recurrence is seldom seen at the antero-medial ends of the intercostal spaces. But the nodes of the internal mammary chain expand, when invaded, in the line of least resistance, i.e. towards the lung.

The only light at present available on the importance of mammary chain deposits is from the fate of the first 50 cases of internal mammary chain biopsy which have now been done three years. That light, owing to the smallness of the numbers and the shortness of the time since treatment, is dim, but here it is (Table III):

TABLE III.—SURVIVAL OF 50 CASES OF CARCINOMA OF BREAST AT 33 MONTHS

	Alive	Dead	Total
All glands free	15	1	16
Axillary glands only invaded	10	6	16
Internal mammary glands only invaded	2	0	2
Internal mammary and axillary glands invaded	5	11	16

These numbers are too small and the time is too short for conclusions, but it is interesting that twice as many patients, whose axillary glands only are invaded, are alive as those in whom both axillary and internal mammary glands are invaded. I wish, however, to point out especially that both the patients in whom only the internal mammary nodes were invaded, are alive and well.

Internal mammary biopsy, in my view, makes nonsense of systems of pathological staging. I think it fair to maintain that, once carcinoma cells have gained a foothold inside the bony cage of the chest a case must be assigned to Stage III. On that basis the effects of my biopsy procedure on the staging of the first 100 cases is as follows (Table IV):

TABLE IV

		Clinically	Pathologically Ignoring int. mammary findings	Pathologically In light of int. mammary findings
Stage I	..	51	36	32
Stage II	..	43	58	34
Stage III	..	6	6	34

The usual inaccuracy in clinical staging is apparent. The difference which the mammary biopsy makes is most striking as between Stage II and Stage III.

A discussion on the practical implications on treatment in the light of these findings would be tentative and controversial. Instead I shall turn to the realm of speculation. The axillary lymphatic pathways are the main drainage channel of the breast. They are, however, provided with an effective filtering mechanism, consisting of scores of lymph nodes. This filter does not adjoin any vital viscera and we can reach it easily by radical mastectomy. We can, in fact, clear it out so effectively that recurrence is unusual in the axilla after radical mastectomy. If the filter is heavily contaminated or has been short-circuited by channels reaching its upper end directly, we are, of course, too late to deal with it effectively, but in early cases we should be able to prevent spread through it. The case is far otherwise with the internal mammary chain. It lies on the pleura and is a main lymphatic road from the liver and diaphragm. It is very difficult to reach surgically, yet it is often invaded before the axilla is heavily implicated. Not only do I think that spread of carcinoma to the internal mammary chain accounts for those puzzling recurrences which occur when a radical mastectomy has been done on an early case, but I also believe that it accounts for the fact that metastasis is commonest in the lungs and pleura and that, in post-mortem series, the liver is the second most commonly invaded organ. I think in fact, that in breast carcinoma, the internal mammary chain is the principal highway of death.

The axiliary and internal mammary lymphatic pathways may not be the only routes open to carcinoma cells of escape from the breast. Where there are blood vessels it is justifiable to postulate the existence of lymphatics. The breast receives some blood from the lateral perforating arteries and there may be lymphatic paths here for spread to the intercostal spaces. I have on several occasions removed sections of the lateral perforating neurovascular bundles but so far Dr. Thackray has failed to find carcinoma cells in them.

I must thank you for listening to me with such patience and remind you that our main purpose this evening is, to " praise famous men and our fathers that begat us ".